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SCIENCE NEWS

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Edited by
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PENGUIN BOOKS

1947

PENGUIN BOOKS LTD.
Harmondsworth, Middlesex, England

PENGUIN BOOKS INC.
245 *Fifth Avenue, New York, U.S.A.*

FIRST PUBLISHED MAY 1947

Photogravure plates by
Eric Bemrose Limited Liverpool

Made and printed in Great Britain
by C. Nicholls & Company Limited
London Manchester Reading

Contents

Many readers were kind enough to write and tell us what they thought of the first issue, and some made suggestions for improvements; the inclusion of an Index in the present and all following numbers is a direct result of this. We are always glad to hear what you think (blame as well as praise !), for only so can we make each issue better than the one before it.

The Testing of Intelligence

DR. ALICE HEIM

I. Introduction

"Intelligence" has been variously defined, by psychologists and others, as "the ability to profit by experience"; as "capacity for abstract thinking"; as "the ability to learn"; as "the capacity to make one's way in the world"; as "the ability to adapt oneself to new situations" and—very similar—as "the ability to solve problems." Some definers have stressed powers of association—the number and relevance of associations formed; others have stressed memory—the capacity for retaining and appropriately recollecting what has been learned. Yet others have hesitated to offer a definition, asserting merely that they use the term "intelligence" to connote "that which is tested by an intelligence test."

Greater agreement, however, obtains among psychologists as to the methods of assessing this ill-defined quality and as to the uses to which such assessment should be put. A study of established intelligence tests reveals that the subject (or candidate) who is to do well requires a high degree of reasoning ability, quick and accurate powers of observation and the capacity to carry out simple instructions exactly.

Evidently the combination of these abilities constitutes something very similar to that which is generally understood by "intelligence." It has been established that people who have them to a marked degree tend to be successful in high-grade occupations, i.e., highly skilled jobs or work requiring clear and concentrated thinking and that such people are liable to become bored and inefficient in routine or over-simple occupations. On the other hand, subjects who gain poor scores on intelligence

should gain an average or near-average score, the others tailing off symmetrically towards the extremes, where a few outstandingly good and outstandingly poor subjects are found.

Normality of distribution is desirable in practice mainly because most of the present statistical methods demand it. It is far more difficult to compare one set of test scores with another, or with some independent criterion of proficiency, if the scores form a very "skew" curve.

It is sometimes maintained that a normal distribution is desirable also on theoretical grounds—that mental traits are in fact normally distributed. There seem to be two answers to this:

(a) The truth concerning the actual distribution of mental traits is not, and cannot be, known.

(b) By sufficient juggling with methods of scoring, almost any set of test scores can be reduced to near-normality.

3. *Consistency*. The test should be consistent (or "reliable"). Clearly an intelligence test which ranks its subjects in a certain order in January and in a quite different order if they take it again in February is a doubtful practical measure of mental ability. Which order is the correct one? Would a third testing produce yet a third rank order?

The most satisfactory method of establishing test-consistency is by test-retest. On this method a group of subjects (at least 100—preferably more) will take the same test twice, with an interval of, say, a few weeks in between the testings. The correlation between the two sets of test scores gives a measure of the degree of consistency, or the "reliability," of the test. The subjects may well improve on their first score; they should, however, improve in such a way that their rank order remains unchanged.

Where test-retest is impracticable the method of "split halves" may be used. Here the test is given once but each subject's score is divided into two parts: either the score gained on odd items may be correlated with the score

gained on even items; or, failing this, the scores obtained by the subjects in the first half of the test may be correlated with those obtained in the second half. A high degree of relationship between the two halves is essential in a consistent test.

4. *Validity*. A valid intelligence test is one which agrees with some independent criterion of the subjects' mental ability—which does in fact test what is intended.

Test validation is as difficult as it is important, owing to the difficulty of obtaining proficiency assessments which are themselves valid. It is rarely possible to find some objective criterion. The assessments are usually in the form of judgments made by one or more individuals, on the subjects who took the test. The judges must be assumed to be perspicacious, unprejudiced, honest, co-operative, intelligent and knowledgeable.

If test scores and assessments of proficiency go well together, it is usually inferred that the test is valid. If the relationship between test scores and assessments is weak or non-existent, it may be inferred that the test is poor, or the assessments are poor, or that the assessments refer to some quality not measured by the test in question.

The validity of a psychological test is obviously one of its most vital features. It is also the hardest to ascertain, owing to the difficulties inherent in obtaining accurate, independent assessments. It has, however, been found possible to increase the value of assessments by the following means: rating scales should be carefully constructed; the assessor should be provided with clear descriptions of the quality he is to judge; he should receive some training in assessing; and two or more independent assessments should be made on each subject.

The above four points are concerned with the more theoretical aspects of intelligence tests. They can be determined only by experimentation: by trying out the test on a suitable group, examining its scores for range,

frequency distribution, consistency and validity, altering the test in accordance with findings, then giving the new version to another group and so on.

There are also a number of more practical points concerning the structure of the test, which the psychologist should bear in mind, during the process of devising the test.

1. *Examples.* Easy examples of the test questions should be provided at the beginning of the test, for the subject to solve in his own time, for practice. If he experiences much difficulty, the tester should help him to understand the problems but should not supply him with the answers. The subject should think these out for himself.

These examples serve several purposes. Working through them acts to some extent as a "shock absorber": the nervous subject may recover from his "examinitis" as he solves these easy problems for himself at leisure, and learns that these are the type of question he will meet in the test proper.

The slow as well as the nervous subject will welcome examples, for they will give him time to accustom himself to the particular conventions used in the test. We refer here to the type of subject who takes a little while to understand what is required of him but works efficiently, and reasonably fast, when he really knows what he has to do.

The frankly stupid subject also benefits from the inclusion of examples. He will still not gain a high score, but he will be less likely to feel resentful of the test than if he were plunged straight into it—in which case he would very likely obtain no marks at all. A score of nought is rarely illuminating. If a test group produces many scores of nought, it may safely be assumed that the test or the tester is open to serious criticism.

The inclusion of examples helps also to counteract, to some extent, the effects of test sophistication and test coaching. In an intelligence test without examples to be worked through, the completely inexperienced subjects are

clearly at a disadvantage, as compared with those who have taken such tests in the past or been coached as test subjects. The examples at the beginning may be regarded as a kind of elementary training for those subjects who are unfamiliar with psychological tests. They help to even things out for the group as a whole.

2. *Order of questions.* As was mentioned above, the questions should be graded in order of difficulty, the first questions being well within the range of capacity of the group and the questions becoming progressively harder throughout the test, in order to minimise the importance of the speed factor.

The relative difficulty of the questions is not of course a matter of the tester's personal opinion. It can be determined only by giving the test to a group with unlimited time and then arranging the questions according to the ratio of correct to incorrect answers, for each question.

Apart from the speed factor, it is better for subjects to work their way through to the harder questions, some of which may depend for their understanding on certain points clearly illustrated in earlier questions.

3. *Incentive.* Unless the co-operation of the subjects is gained, the test results will be of little value. Psychological tests provide full scope for leg-pulling, for slacking and for cheating—unless the test is devised, and the tester trained, with these contingencies in mind.

In an intelligence test it is a good idea to vary the subject matter and the types of logical principle underlying the questions as much as possible. The subjects will be less likely to become bored or antagonistic if they meet variety, and especially if they find some suggestion of humour in the test or in the tester.

The technique of the tester plays an important part in determining the success or failure of a psychological test. The best of tests will produce odd results if the tester is not himself familiar with the test or at ease in the test situation, gives unclear, ambiguous or insufficient

instructions, stands over subjects while they are working, or for any other reason fails to gain their confidence and interest.

III. Differences between Psychological Testing and other Methods

Psychological tests differ from other methods of estimating mental capacity in two respects: they give a measure of *potentiality* or teachability and they are wholly *objective.*

Let us compare them first with examinations and "general knowledge quizzes." Both these aim at assessing acquired knowledge. Success in examinations depends largely on the memory of the candidate, the teaching he has received, the conscientiousness with which he has worked and his luck in the papers. The lack of agreement among examiners, especially in arts subjects, is well known.

"General knowledge quizzes" avowedly aim at testing knowledge again; they are inevitably selective rather than general in their subject matter; they often include "catch" questions (which are bad for the morale of the subject in addition to being unenlightening to the questioner) and their results are very dependent on the particular education the answerer has received.

The interview, perhaps the most frequently used method of estimating the mental level of people for practical purposes, is of course highly subjective and unreliable. There tends to be far less agreement among interviewers (when any check is made) than among examiners, and the self-consistency of the interviewer is liable to be low.

These findings are to be expected, since interviewing is an intrinsically unreliable method of personal assessment, even when it is, as rarely, competently conducted. The personalities of the two people (more, in the case of a Board interview) react on one another and the candidate may in fact display a particular quality to a high degree with one interviewer, and very slightly with another.

Certain social and temperamental traits can probably be

assessed by a good interviewer more satisfactorily than by any other method, at present, but intelligence is not amenable to assessment by this method.

Good intelligence tests are not open to the above criticisms. They are objective in the sense that the questions and answers are capable of one legitimate interpretation only and that the scoring of the answers is strictly uniform. Degrees of rightness are eliminated: the answer submitted is either right or wrong and there is only one possible correct answer. Thus any number of competent testers, using the same test on the same group, would obtain almost identical results. This is true also of the same tester, testing the same group at a different time.

Intelligence testing, unlike examinations and general knowledge quizzes, is concerned with *innate* capacity, i.e., the abilities tested are not to any marked degree dependent on the external environment or the education of the subject. As outlined in the Introduction, they comprise reasoning, observation and response to instructions—fundamental qualities whose presence or absence can be disguised only with extreme difficulty.

Some paper and pencil intelligence tests do not involve any reading at all, whilst others—those with a verbal bias—demand a low standard as far as the actual reading is concerned, far lower than is possessed by the average literate adult.

Intelligence tests are not designed to give any information as to the *creative* abilities of the subject. The eliciting of such information is probably incompatible with the "choice-of-answer" system, by which objective marking is achieved. Nor, of course, do intelligence tests yield information on the temperament or character of the subject.

Psychological tests do not give the whole story, even when tests of specific aptitudes are added to those of intelligence. In order to obtain a complete picture of an individual, he should be interviewed as well as tested, and examined (in some cases for health, as well as for know-

ledge or skill). His previous training and experience should be considered.

Psychological tests are most valuable when they conflict with one or more of these other data, especially when the subject does *better* on a test than might have been expected on other grounds. It may safely be assumed that he is at least as intelligent as his test score suggests: he may be more so but it is very unlikely that he will be less so.

IV. Some Criticisms Answered

The most common objection to intelligence tests, that they merely test speed, has been discussed above. Other frequently met criticisms include the following:

1. Test results cannot be taken seriously since the tests tend to antagonise or frighten or amuse people.
2. It all depends *when* the test is taken: subjects' scores will vary according to their state of mind and of physical health at the time of being tested, according to the weather, the season and the time of day; according to home circumstances and the international situation; and different subjects will be differently affected by these conditions.
3. Any paper test is a test of education and it is not fair to penalise the people who have not had the chance of a good education. This is related to objection 4.
4. Everyone should have an equal chance. Tests give grounds for unfair discrimination between people; they should all be treated alike.
5. "I can sum up a fellow when he enters the room—it's much quicker than giving a whole test. Intelligence tests are a waste of time."

Let us discuss these criticisms in turn. The first two have occurred even to psychologists and their importance has been examined experimentally. As was suggested in Section II, the design of the test and the technique of the tester can go a long way towards minimising inappropriate emotional reactions. In fact some tests do give reliable results and others do not. The value of any particular test

can be determined only by experimentation, not by theorising as to what must occur and what effects these occurrences will have.

The part played by individual variation in psychological tests appears to be surprisingly small. The tester is often assured by his subjects that they would have gained a different (and always a better !) score if they had taken the test at 11.0 a.m. instead of 3.0 p.m.; if they had not read the newspapers on the day of the test; if they had refused the plum pudding at lunch or the pub crawl the preceding evening.

It would be foolish to deny that these factors exert some influence on the test scores—just as foolish as to deny that they play their rôle in determining examination results and interview decisions. There is some evidence, however, that the differences between individuals on a reputable psychological test are for the most part greater, and more significant, than differences between the same person at different times.

The criticism that a subject's intelligence test score depends entirely or largely on his education is generally based on ignorance of test material and results. Test scores are less a function of the subject's education than are examination results, always, and interview assessment, often. It is true that a positive correlation obtains between type of education and intelligence test score, but this is due mainly to the existence of a reasonably efficient scholarship and free place system. The notable exceptions—the labourer who left school at fourteen, some years back, whose intelligence test score is well above average—and the university graduate whose intelligence is barely average —confirm the psychologist's contention that intelligence tests do not assess the educational level of the subject but his innate capacity.

Criticism 4 is, in our view, based on two implicit assumptions: (a) that everyone is in fact equally gifted, (b) that without tests, there is no "discrimination." The falsehood of (a) is patent to most people who have devoted any

thought to human problems and to everyone who has done any kind of teaching or training of children or adults.

With regard to (b): there is of course "discrimination" where tests are not used, but it is less fair to the individual and more wasteful to the community. The use of psychological tests brings us much nearer to *equality of opportunity* than any other method.

Criticism 5: summing up a fellow the moment he enters the room is certainly far quicker than administering an intelligence test, and this school of thought is by no means extinct. Its adherents are usually difficult to convince of the error of their ways because they tend not to keep follow-up records—"what is the point?—more waste of time—we know the answer." They would probably admit, however, that interviewers do not always see eye to eye. If pushed, they might agree that very few interviewers have this flair, or even that they personally happen to be uniquely gifted.

V. Intelligence Testing and Vocational Psychology

Vocational psychology includes choosing the man for the job (vocational selection) and choosing the job for the man (vocational guidance). Intelligence testing plays an important part in both since it gives the vocational adviser a good idea of the general level of the candidate's mental ability. Once this is known, a great many occupations can be dismissed as not worthy of consideration.

By means of testing large numbers of people in different occupational groups, psychologists have determined roughly the level of intelligence required by different employments. Each level naturally includes many different types of work and there is considerable overlap between the intelligence levels of the various groups of occupation.

Nevertheless, intelligence is a good starting-point for vocational advice since an appropriate amount is necessary (though not, of course, sufficient) for success in every occupation. The man who lacks the innate ability will not

make a good draughtsman, for instance, however keen his ambition and however conscientious his work. Given that his intelligence comes within the range of scores obtained by draughtsmen, it is then worth while investigating the other relevant factors.

These include his specific aptitudes—can he draw? has he good visual perception? His temperament and character traits—will he stick at it? has he the patience? and the stability? His educational and employment attainments—what standard of mathematics has he achieved? did he do any mechanical drawing in his last post? His home environment—can his parents afford the time and money he will need for training? His interest—how does he spend his spare time? His own vocational desires—does he want to become a draughtsman? if so, why? if not, what would he like? His physique—would it suit him to spend most of his time sitting indoors? will his eyes stand the strain of fine work?

All these factors are important, but if the potential draughtsman has sufficient intelligence he may be able to compensate for some deficiency in, say, personality, aptitude or physique. It is extremely rare, however, for the unintelligent candidate to make good in a skilled occupation, however hard he may try and however well backed he may be by his parents. Intelligence is the most important *single* factor to take into account when giving vocational advice, and the easiest to assess quickly and reliably.

SUGGESTED READING

Burt, Cyril: *Mental and Scholastic Tests.*

Knight, Rex: *Intelligence and Intelligence Tests.*

Macrae, Angus: *Talents and Temperaments.*

Bartlett, Ginsberg, Lindgren & Thouless: *A Study of Society, Part II: Social Applications of Psychological Tests and Other Methods.*

Synthetic Emeralds

G. VAN PRAAGH

THE emerald is probably the most valued gem-stone; in 1933, for example, fine cut emeralds were about three times the price of diamonds. The value of a precious stone is, of course, liable to change once a synthetic material can be produced, and, if it is difficult to distinguish the natural from the synthetic product, then the effect on the value of the natural stone is likely to be great. By a synthetic mineral we do not mean an artificial imitation made of some other material that closely resembles the natural substance, but we refer to a material identical in every respect with that occurring in the earth's crust except that it has been manufactured in the laboratory.

Many attempts have been made to produce synthetic diamonds, dating back to about 1820, the best known being that by H. Moissan in 1896. The problem here is to persuade carbon to crystallize in the close-packed hexagonal form known as diamond. Moissan heated carbon dissolved in iron in an electric arc furnace, and the high pressure developed on cooling caused the carbon to separate in a form which he believed to be diamond. It is now probable that this was in fact not diamond at all but iron carbide. Subsequent attempts to make synthetic diamonds suitable for industrial purposes have been successful, but it remains true that no sizeable brilliant diamonds, comparable with the natural gems, have been produced.

Rubies and sapphires have for some years been successfully synthesized on a large scale, and before the war, French, Swiss and German firms were making material identical with the natural stones. These gems are transparent forms of the mineral corundum (aluminium oxide), ruby being coloured with chromium and sapphire with

titanium. Owing to their great hardness (9 on Moh's scale compared with 10 for diamonds) they are the jewels used as bearings in watches, clocks and a number of delicate electrical instruments, and it was for this purpose that large quantities of the synthetic material were made. During the war, plant for production of synthetic rubies and sapphires was built in Britain. The principle of the method of making the material is to heat aluminium oxide to very high temperatures at which it melts, and to control the cooling so that a large crystal is built up as the molten material solidifies.

Emeralds are more complex chemically than rubies and sapphires. The latter consist of a simple oxide, whereas emeralds are a form of the mineral beryl, beryllium aluminium silicate. One method available for crystallizing such a substance would be to find a solvent in which it is more soluble at high than at low temperatures. Emerald would normally be regarded as extremely insoluble in water. However, a German mineralogist, Professor Nacken, of Frankfurt University, started in 1912 to work on the problem of synthesizing these and other similar insoluble minerals. His method was to attempt to synthesize these minerals by crystallizing them from water or dilute aqueous solution in the neighbourhood of the critical temperature* of water. By 1928 he had succeeded in synthesizing a number of minerals including felspars, mica and beryl. He was eventually able to make synthetic emeralds of about 1 carat (0.2 grams) which are identical in every way with the natural gem-stone, and to do it in the course of a few days.

The method employed was as follows:—

Crystals were grown on to a "seed" crystal in much the same way as one can grow crystals of common salt from brine. Nacken made use of the fact that the solubility of these minerals becomes appreciable at high temperatures and pressures, and he used a technique for crystallizing them from solution by sealing up the requisite raw materials

* See Glossary.

in an autoclave which was then heated to the critical temperature of water. The autoclaves were made of steel and consisted of a cylindrical vessel of about 30 ml. capacity with thick walls, closed at one end with an internal screw plug. The vessel was lined with silver and the seed crystal was suspended from a silver wire attached to the plug. The solvent, which was water containing traces of weak alkalis, occupied about 20% of the volume at room temperature. The raw material, consisting of a mixture of beryllium oxide, aluminium, silica in the correct proportions, was placed in the autoclave, which was then closed and the temperature raised to about 370–400° C. It was maintained at this value for a few days. Under these conditions the water is in the neighbourhood of the critical point and completely fills the vessel. A very large number of experiments were needed to determine the best conditions, but finally Nacken was able to make crystals of beryl (coloured green with a trace of chromium to turn them into emeralds) that were up to 1 cm. in length and 2 to 3 mm. in width. Four specimens are shown in Plate I. The only way in which they are likely to differ from the natural gem-stone is that they will be free from minor faults and inclusions which always characterize naturally occurring minerals.

*The History of Blood Transfusion, 1628-1914**

GEOFFREY KEYNES

BLOOD transfusion for therapeutic purposes is now one of the commonplaces of medical practice. Yet it is less than twenty-five years since it came to be recognised as a practicable procedure of wide application, and it is of value to retrace in clear outlines the stages by which this immensely important advance came about; and to understand why its evolution was so slow, although its value in saving human life is now so obvious. In the present account particular attention will be given to the earlier efforts of our predecessors in medical science, interest being often stimulated more by knowledge of the work of pioneers and originators than by that of late-comers, even though it is commonly the refinements of knowledge and the perfections of practice that are the more difficult to achieve.

From the dawn of human history the vital importance of the blood in the animal economy has been fully appreciated, so much so that it was usually invested with many mysterious properties, and was supposed to carry with it the characters of its owners, both mental and physical. All the classical and medieval references to the therapeutic value of blood are more related to magic than to medicine, and this continued until well into the seventeenth century, so that the true history of blood transfusion does not begin until a relatively late date—it could not, indeed, begin until the minds of the medical profession had seized the conception of the circulation of the blood, enunciated and published by William Harvey in 1628. Although Harvey had himself pumped water through the circulation of a dead

* A Friday evening discourse, delivered at the Royal Institution, April 9, 1943. Reprinted by courtesy of the British Journal of Surgery.

man, there is no evidence that he ever considered blood transfusion in relation to his medical practice. In the same year, 1628, a Professor at Padua University, Johannes Colle, who may have known before of Harvey's work, did seriously suggest transfusion as a practical proposition, though he did not perform it. During the next twenty-five years the conception of the circulation was sinking in, but it was not until after 1655 that the scramble for priority can really be said to begin in relation to the practice of transfusion.

There were several phases to be gone through: (1) The formulation in men's minds of the *idea* of introducing other fluids and drugs in solution into the circulation of an animal; (2) The extension of the idea to include the introduction of blood; (3) The preliminary experiments on the circulating blood of animals; (4) The application of the results to the therapeutic use of blood transfusion in man. It was only to be expected that several minds in different countries of Europe should be thinking along parallel lines, and so giving rise to conflicting claims when it came to deciding the question of priorities. As an example of this process we may first examine the writings of a Florentine physician, Francesco Folli, who published in 1680 a book setting out his claim to be the originator of blood transfusion. He stated that he read William Harvey's treatise on the motion of the heart in 1652, and thereupon formed in his mind the idea that transfusion of blood should be possible to cure diseases and to rejuvenate the aged. He recalled that he had seen the precedent in 1645 when he observed in Pisa a double monster in the shape of male twins joined together by their livers and surrounding parts of their bodies. Only one of the twins was able to eat and drink, the other being in many ways imperfect, so that it was dependent for its nourishment on the food taken into the stomach of its brother. Nature had thus provided an example of blood transfusion, one stomach nourishing two bodies through a common circulation. Harvey's treatise clinched the matter in Folli's mind, and he goes on: "This

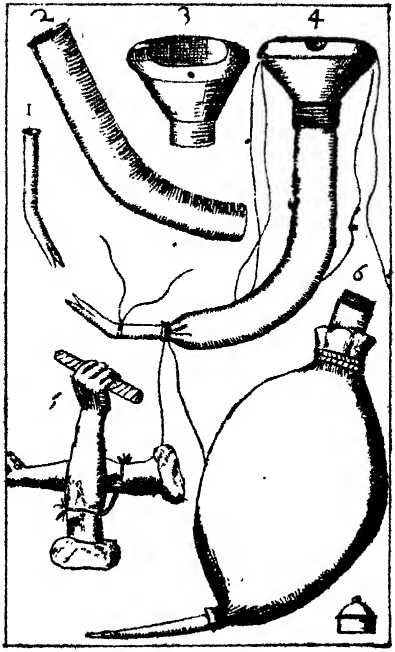

Figure 1

I pointed out in my pamphlet on life culture, which was published for no other reason but to make known to all that blood transfusion had been invented by me at the end of 1654 and demonstrated to his Serene Highness Ferdinand II, Grand Duke of Tuscany, of undying memory. The novelty of it had pleased him, or the fascinating ingenuity or the considerable experimental elaboration. To no one else did I impart my idea, believing that if such an invention were successful, Monarchs alone were worthy of it."

Later in the book Folli describes in some detail the apparatus required and the method of using it. He even

postulates the presence of twenty young men as blood donors, so that the patient may receive every day the blood of a fresh donor over a considerable period. The illustration (Fig. 1) shows his apparatus, consisting of a funnel connected by a tube formed from a goat's artery with a gold or silver cannula to be inserted in the patient's vein. It is all very ingenious, but near the end of the book comes the confession which spoils it all. "Finally," he says, "I know that I have said too much concerning the manner of carrying out the operation, not having made the experiment, . . . but I have done it solely so that everyone, however simple and ignorant, could understand, be inspired, and even make the experiment with the least possible expense, and to this end only I have written in the vulgar tongue." Folli did, indeed, decorate his portrait in his book with the emblems of his supposed discovery, but it all existed only in his imagination and his claim to be a pioneer in the practice of blood transfusion can be firmly rejected.

Another more genuine, though pathetically unsuccessful, attempt at transfusion was made by the eccentric Vicar of Kilmanton in Somerset, Francis Potter. It was stated by Anthony Wood that he had the idea of curing diseases by blood transfusion as early as 1640, but he chose the hen for his experiments. Later he wrote to John Aubrey that "I am as yet frustrated *in ipso limine*, but it is by my owne unexpertnes, who never attempted any such thing upon any creature before: for I cannot, although I have tried divers times, strike the veine so as to make him bleed in any considerable quantity.

"I have prepared a little cleare transparent vessel (like unto a bladder) made of the craw of a pullet; and I have fastened an ivory pipe to one of the neckes of it, and I have put it into a veine which is most conspicuous about the lowest joint of the hinder legges; and yet I cannot procure above 2 or 3 drops of blood to come into the pipe or the bladder." This was written in December, 1652, but it

cannot be claimed that the Reverend Francis Potter had greatly advanced the art of blood transfusion.

We may pass over other claims with equally, or even more, slender foundations, and turn to consider the more objective claims of the true originators of blood transfusion working in England and France. The first experiments are to be attributed to the ingenious mind and clever hands of Dr. (afterwards Sir Christopher) Wren. Dr. Thomas Sprat, in his *History of the Royal Society*, 1667, records that "he [Dr. Wren] was the first Author of the Noble Anatomical Experiment of Injecting Liquors into the Veins of Animals: An Experiment now vulgarly known; but long since exhibited to the Meetings at Oxford, and thence carried by some Germans, and publish'd abroad. By this Operation Creatures were immediately purg'd, vomited, intoxicated, kill'd, or reviv'd according to the quality of the Liquor injected. Hence arose many new Experiments, and chiefly that of Transfusing Blood, which the Society has prosecuted in sundry Instances, that will probably end in extraordinary Success." This was in 1657, and Wren's experiments were described by the great Robert Boyle in his *Usefulness of Experimental Philosophy* published in 1663. The Royal Society had been started in London in 1661 and received its Royal Charter in 1662, and it was this "philosophic assembly" of distinguished scientists that witnessed the beginnings of blood transfusion. Among several persons concerned in the experiments, Richard Lower, then a doctor practising in Oxford, claims first place, though preliminary suggestions and experiments had been made by others. This is revealed by entries in the Journal Book of the Royal Society (quoted by Aveling):—

May 17, 1665. "It was suggested by Dr. Wilkins that the experiment of injecting the blood of one dog into the veins of another might be made."

May 24, 1665. "Mr. Thos. Cox related that he had made an experiment of injecting the blood of one pigeon into the vein of another."

May 31, 1665. "Dr. Croon suggested that a common pipe might be used for both, thereby to have the bloud of one dog suckt out by the other." (This is the first mention of direct transfusion.)

June 7, 1665. "Dr. Wilkins reported that he by means of a small pipe of brass and a bladder injected two ounces of blood into the crural vein of a bitch."

Robert Boyle had himself performed a similar operation, and on April 18, 1666, he declared that he thought Dr. Lower would be willing to prosecute the experiments at Oxford.

Lower's celebrated book on the heart, published in 1669, proves him to have been one of the great pioneers in anatomy and physiology. In this book he states that he proposes "to reveal the conduct of the whole affair [of blood transfusion] and at the same time to show by what train of thought [he] first reasoned it out and undertook it, and, finally, by what means and aids it was carried into effect." He goes on to say: "For many years at Oxford I saw others at work, and myself for the sake of experiment injected into the veins of living animals various opiate and emetic solutions, and many medicinal fluids of that sort. . . . But when, in addition, I likewise injected many nutrient solutions, and had seen the blood of different animals mix quite well and harmoniously with various injections of wine and beer, it soon occurred to me to try if the blood of different animals would not be much more suitable and would mix without danger or conflict. And because in shed blood . . . the natural blending and texture of the parts must of necessity change, I thought it much more convenient to transfer the unimpaired blood of an animal which was still alive and breathing into another. I thought this would be more easily effected, inasmuch as the movement of blood through its vessels is so rapid and swift, that I had observed almost the whole mass of blood flow out in a few seconds where an outlet offered. Taking hope from this, I turned mind and hands to put the matter to a practical test.

"And first I tried to transfer the blood from the jugular vein of one animal to the jugular vein of a second by means of tubes between the two; but, seeing the blood clot at once in the tube and block its own passage on account of the slow passage of the venous blood, I soon began to try another way, and guided, as it were, by nature herself, I finally determined to transfer blood from an artery of one animal into the vein of a second; and by this new device to extend the circulation of the blood beyond the boundaries prescribed for it.

"As everything answered expectation as I wished, I finally showed this new experiment at Oxford towards the end of February, 1665 [1666 new style], in an interesting demonstration and under the most happy circumstances. . . .

"Having got ready the dogs, and made other preparations as required, I selected one dog of medium size, opened its jugular vein, and drew off blood, until it was quite clear from its howls and struggles that its strength was nearly gone and that convulsions were not far off. Then, to make up for the great loss of this dog by the blood of a second, I introduced blood from the cervical artery of a fairly large mastiff, which had been fastened alongside the first dog, until this latter by its restiveness showed in its turn that it was overfilled and burdened by the amount of the inflowing blood. This was repeated several times in succession, until there was no more blood or life left in two fairly large mastiffs. . . . In the meantime blood had been repeatedly withdrawn from this smaller animal and injected into it such amount as would equal, I imagine, the weight of its own body, yet, once its jugular vein was sewn up and its binding shackles cast off, it promptly jumped down from the table, and, apparently oblivious of its hurts, soon began to fondle its master, and to roll on the grass to clean itself of blood; exactly as it would have done if it had merely been thrown into a stream, and with no more sign of discomfort or of displeasure."

Lower's account of this is given at length because it is the first description of a successful direct transfusion from artery to vein, and so is of paramount importance in the present history. Lower illustrated his apparatus in an

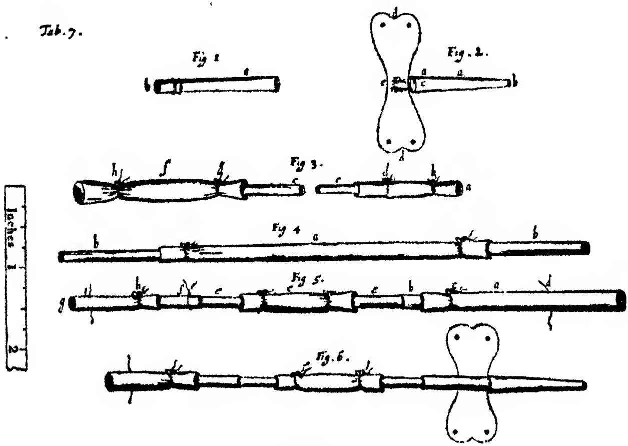

Figure 2

engraved plate (Fig. 2). He had at first used quills for uniting the blood-vessels of the two dogs, but afterwards decided that silver tubes could be more securely fixed in the emitting and receiving blood-vessels, these tubes being connected by a piece of the cervical artery taken from an ox. They were provided with small flanges so that they could be tied in more securely. The tube with a plate attached was devised for inserting into the arm vein of a human being.

These experiments were the subject of a series of letters between Lower and Boyle, and were described in No. 20 of the *Transactions* of the Royal Society, December 17, 1666.

Lower was at great pains to establish the facts of his experiments and their dates because, as he complained, "a certain Denis" was seeking to deprive him of priority in

the matter and to claim it for himself. This was Jean Denys, Professor of Philosophy and Mathematics at Montpellier, and physician to Louis XIV. Denys wrote a long letter from Paris in June, 1667, which was translated into English and actually printed as No. 27 of the *Transactions* of the Royal Society for July 22 of that year. It appears, however, that the printing was done without the knowledge of the Secretary, Henry Oldenburg, who happened to be in confinement in the Tower of London at the moment. Oldenburg was, however, released just in time to have the letter suppressed so that it is not included in most existing sets of the *Transactions*. It is now of great rarity.* Parts of it were published later. In this letter Denys describes his preliminary experiments on animals which were similar to Lower's, though certainly performed a year later, and explains how these led to a resolve to extend them to transfusing the blood of one kind of animal into the veins of another kind. In March, 1667, blood of a calf was therefore transfused into the veins of a dog, apparently without ill effect. Denys then argues at great length that it would be expedient to use the blood of an animal for treatment of disorders in man, and described how on June 15, 1667, he happened on a youth of 15 who had for months been tormented with a fever, for which he had been bled by his physicians twenty times "to assuage the excessive heat." "Before his disease, he was not observed to be of a lumpish dull spirit, his memory was happy enough, and he seemed cheerful and nimble in body; but since the violence of his fever, his wit seem'd wholly sunk, his memory perfectly lost, and his body so heavy and drowsie that he was not fit for anything." Accordingly he was bled to the extent of about 3 oz., and received in exchange about 9 oz. from the carotid artery of a lamb. The change that ensued was startling, and presently the boy was showing "a clear and smiling countenance" where previously he had passed

*Copies are to be found in the library of the Royal Society, in Dr. John Fulton's library at Yale, and in my own.

the time in an incredible stupidity." He had felt "a very great heat along his arm," which is the usual sign of an incompatible blood, but there were no further ill effects.

This boy had been transfused for therapeutic purposes. Denys did his second transfusion upon an older man "having no considerable indisposition," and it was purely experimental. The subject was supposed to have received about 20 oz. of lamb's blood, but again there were no ill effects, and it may be doubted whether the man received as much as this. Indeed "he abated nothing of his jovial humour" during the process, and when it was over zestfully cut the throat of the lamb and fleeced it, that having been his profession all his life! Denys afterwards performed further transfusions, and he claimed to have cured a patient suffering from "an inveterate phrenzy." On this occasion the blood of a calf was used, and there can be no doubt that the patient received a considerable amount, for he showed all the signs of receiving incompatible blood—pain in the arm, a rapid and irregular pulse-rate, sweating, pain in his back, vomiting, and diarrhœa; he afterwards passed urine that was almost black with the hæmoglobin of destroyed blood-cells. In fact, he was fortunate to have escaped with his life.

Meanwhile in England the Royal Society circle had been working up to the point of transfusing a human being. Our best reporter is Samuel Pepys, who wrote in his Diary on November 14, 1666: "Here [at the Pope's Head] Dr. Croone told me, that, at the meeting at Gresham College tonight, . . . there was a pretty experiment of the blood of one dogg let out, till he died, into the body of another on one side, while all his own run out on the other side. The first died upon the place, and the other very well, and likely to do well. This did give occasion to many pretty wishes, as of the blood of a Quaker to be let into an Archbishop, and such like; but, as Dr. Croone says, may, if it takes, be of mighty use to man's health, for the amending of bad blood by borrowing from a better body." Other

conversations followed, and a year later, under the date November 21, 1667, Pepys wrote: "Among the rest they discourse of a man that is a little frantic, that hath been a kind of minister, Dr. Wilkins saying that he hath read for him in his church, that is poor and a debauched man, that the College have hired for 20s. to have some of the blood of a sheep let into his body; and it is to be done on Saturday next. They purpose to let in about twelve ounces; which, they compute, is what will be let in in a minute's time by a watch. They differ in the opinion they have of the effects of it; some think it may have a good effect upon him as a frantic man by cooling his blood, others that it will not have any effect at all. But the man is a healthy man, and by this means will be able to give an account what alteration, if any, he do find in himself, and so may be usefull . . ." On November 30 Pepys dined at a house of entertainment, and enjoyed good company. "But here, above all, I was pleased to see the person who had his blood taken out. He speaks well, and did this day give the Society a relation thereof in Latin, saying that he finds himself much better since, and as a new man, but he is cracked a little in his head, though he speaks very reasonably and very well. He had but 20s. for his suffering it, and is to have the same again tried upon him: the first sound man that ever had it tried on him in England, and but one that we hear of in France, which was a porter hired by the virtuosos."

The subject of this experiment was one Arthur Coga, aged about 32, an indigent Bachelor of Divinity of Cambridge, and brother to the Master of Pembroke. It is recorded in the *Philosophical Transactions* that the experiment was performed by Drs. Richard Lower and Edmund King at Arundel House on November 23, 1667, that is, over six months after the first transfusion experiment on a human being had been done in Paris. It must be conceded, in fact, that, although Lower had the priority in animal to animal transfusion, Denys had the priority in animal to man. This cause for congratulation, however, did not last

him for long, for in the next year, 1668, one of his patients died, after the third of a series of transfusions, and the widow instituted proceedings against him. The case aroused great feeling, and ultimately a verdict was given against Denys. It was directed that in future no transfusion was to be performed without the permission of a member of the Faculty of Medicine of Paris. As the Faculty was bitterly opposed to the whole idea, this permission was never given, and in 1670 an Arrêt forbade the practice of transfusion altogether. The disaster which had overtaken Denys no doubt had its repercussions in London, and although the Journal Book of the Royal Society records that, as foretold by Pepys, Arthur Coga underwent a second transfusion without serious effect on December 14, 1667, this account was never published in the *Transactions*, perhaps because, as reported by Sir Philip Skippon in a letter to John Ray, "the effects of the transfusion are not seen, the coffee-houses having endeavoured to debauch the fellow, and so consequently discredit the Royal Society and make the experiment ridiculous." Indeed, nothing more was heard of transfusions in England after a few further animal experiments had been done in 1669.

Nevertheless the noble experiment of transfusion in man had gained great notoriety, and some of the authors of text-books felt that they had to include an account of the procedure in their writings, though without inquiring too closely into the therapeutic results. One of the earliest illustrations (Plate 7) was published by Elsholtz in his little book, *Clysmatica Nova*, in 1667. He was very quick off the mark in giving an account of the experiments carried out in various countries.

Another representation (Plate 6) is that given by Purmann in his surgical text-book published at Frankfurt in 1705. Experiments had, of course, been done in Germany as well as in France and England, and the author had seen a transfusion done at Frankfurt in 1668, when a young man received lamb's blood with benefit. The Frankfurt lamb

seems to be controlled almost solely by an admonishing finger, and no doubt Purmann's memory of the incident had again idealised the difficulties out of existence, though he does remark that usually transfusion helps but little.

Now that we know more of the grave dangers attending the transfusion of more than a small quantity of the blood of any animal into man we feel no surprise at the disrepute into which the operation fell after the year 1668. Animal blood, whether it be of dog, lamb, or calf, contains proteins which are totally incompatible with those in human blood. The transfused blood-cells are rapidly destroyed and the symptoms of anaphylaxis follow, causing the death of the patient if the amount of foreign protein introduced is large enough. Little, therefore, is to be recorded of blood transfusion throughout the eighteenth century, though towards its close, in 1794, the active mind of Dr. Erasmus Darwin was occupied with its possibilities. In his *Zoonomia* he suggested blood transfusion for the treatment of putrid fever and cancer of the œsophagus, and for other conditions of impaired nutrition. He described the transference of blood from donor to recipient through goose quills connected by a short length of chicken's gut, which could be alternately allowed to fill from the donor and emptied by pressure into the patient. Again, however, this remained in the realm of good ideas, the operation never being actually performed. It was not until well into the nineteenth century that any serious revival of the operation was attempted.

We are now approaching the real crisis in the history of blood transfusion, the moment when the first transfusion in the modern sense was performed. During the seventeenth century there had been much playing with ideas, and in the earlier stages, as related, attempts had been made to use the blood of animals therapeutically, though usually, be it noted, to treat senility, insanity, or chronic diseases, rather than to meet the emergency of severe blood loss. Partly this may have been due to the difficulty of arranging an animal transfusion at short notice, but more, perhaps, to the pre-

occupation of the medical profession with the supposed mental or semi-magical effects of transfused blood rather than with its value in replacing blood that had been lost. The hero of this critical period was James Blundell, a noted physician, physiologist, and obstetrician, born in 1790. From 1814 to 1836 he was lecturer to Guy's and St. Thomas's Hospitals, and during this period he made notable contributions to abdominal surgery and to knowledge of blood transfusion. The *Dictionary of National Biography* records that he left at his death in 1877 a fortune of £350,000, but makes no reference to his work on transfusion. He was, nevertheless, a pioneer in this field, and he was stimulated to make his researches by realisation of his helplessness in face of the severe and often fatal hæmorrhages which might follow childbirth. As he rightly observed in 1834, "for the original operation the presence of some animal in the bed-chamber was necessary; what, then, was to be done in an emergency? A dog, it is true, might have come when you whistled, but the animal is small; a calf or a sheep might, to some, have appeared fitter for the purpose; but then it could not run upstairs. In this condition of it, the operation, little more than a name, was great in its danger, but of small advantage in those very cases of sudden bleeding in which it seemed most required."

At the same time he was saved from making the bad initial error of his predecessors by the observation of a colleague, Dr. Leacock of Barbados, that the blood of animals might be harmful to human beings, or indeed that the blood of any one species might not serve for any other species. He therefore carried out a long series of experiments on dogs, showing firstly, like Lower before him, that an animal which had been bled almost to the point of death might be revived and restored to health by the blood of another dog, thus proving his initial contention that human lives might be saved by blood transfusion. He then showed that, if the exsanguinated dog were transfused with the blood of a sheep, it invariably died, even though there was

a partial recovery at the start. These experiments sounded the warning against the transfusion of animal blood into human veins, and first established the modern doctrine of incompatibility of the bloods of different species.

The historic date to be assigned to the first transfusion of human blood is December 22, 1818, on which day Blundell read to the Medico-Chirurgical Society an account of the transfusion which he performed with the help of a celebrated surgeon, Henry Cline. The patient was moribund from inanition due to obstruction of the outlet of the stomach. Blundell thought that he might benefit from a transfusion, and that in any event no harm would be done by the experiment. The man received 12 to 14 oz. of blood from several donors by means of the syringe in the course of 30 to 40 minutes, and he temporarily improved, though he soon relapsed, and died 56 hours after the transfusion His disease was incurable, and nothing could really have been expected from the transfusion, but evidently Blundell was determined to start cautiously. When reading his paper he apologised for being prolix about a single operation, but justified the recording of every detail by the fact that it was a new departure. He was conscious, in fact, of the historic importance of the occasion, though he can scarcely have anticipated the time when transfusions were to be numbered by thousands and perhaps by millions. After 1818 Blundell performed more transfusions as occasion offered. In 1824 he recorded six operations, including the one already mentioned, chiefly for exsanguination, but his attempts were uniformly unsuccessful in saving life owing to the fact that at least two of his patients were already dead when he transfused them, and the remainder too ill for recovery to be possible. This was excessive caution, and statistically his operation failed, but he had established a precedent and a principle which was all-important. He persevered, and performed further transfusions. The record of a successful transfusion by Blundell is printed in *The Lancet* for 1829. It was done for hæmorrhage after child-

birth, the patient receiving 8 oz. of blood from the arm of his assistant during the course of three hours, and making a good recovery. It is claimed for him that he performed ten transfusions in all, of which not more than four can have been successful. Allowing for the two patients who were dead before he began, this means four successes out of eight attempts. Blundell was undoubtedly the first operator to transfuse human blood into human patients, and to him all honour is due.

It is interesting to make some note of how Blundell carried out his transfusions. At first he used a simple form of brass syringe and cannula, sucking blood out of the donor's vein and then injecting it into the patient's vein. But this did not satisfy him, so he invented a strange and clumsy-looking instrument called an "impellor," which was funnel and pump combined, as may be seen in the diagram

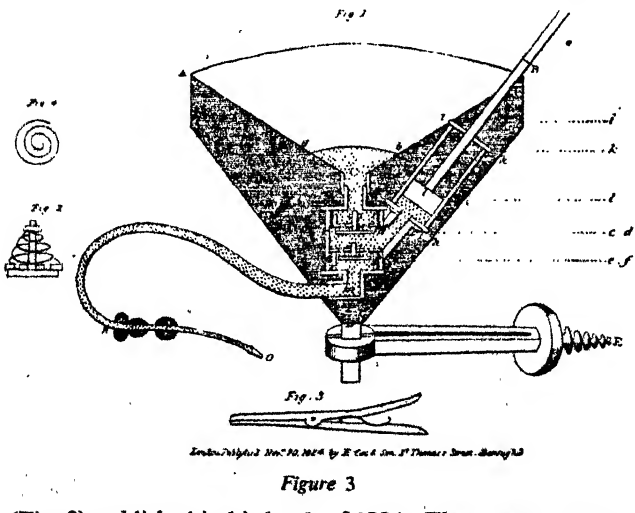

Figure 3

(Fig. 3) published in his book of 1824. The outer compartment, shaded in the diagram, was filled with warm water.

The donor's blood was made to flow into the funnel-shaped part above, and the action of the pump at one side forced the blood along the tube to the cannula inserted in the patient's vein by means of two oppositely-acting spring valves below the funnel. The impellor was fixed to the back of a chair to give it stability, and also to accommodate the blood-donor while blood was flowing from his arm into the funnel.

Later he invented another instrument, the gravitator, in which gravity provided the motive force for pushing the blood into the patient's vein. This consisted of a funnel at the end of a long flexible bracket connected by a tube with a cannula which was buckled to the patient's arm. The

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Figure 4

other end of the bracket was again fixed to a chair (Fig. 4), but this time the blood donor had to stand, while he watched his blood gushing into the funnel.

Blundell was at pains to show by experiment that the blood was not injured by its passage through an instrument, and that the introduction of a few air bubbles into the circulation was quite harmless. Earlier observers had thought that the smallest amount of air in the circulation was rapidly fatal. Blundell thus established so many fundamental points that it is difficult to exaggerate the importance of his work in the history of blood transfusion,

Blundell also collaborated with two other experimenters. Doubleday and Waller, who worked with a much larger animal, the horse. One of their experiments consisted in connecting the jugular vein of one animal with the carotid artery of another by means of a long elastic tube with a stop-cock at either end near the blood-vessels. One horse was then bled until it showed the signs of impending death—the stop-cocks were opened so that the blood flowed from the second horse into the first, which quickly recovered. This established, not only that recovery could take place after extreme exsanguination, but that a relatively small amount of blood was needed to effect it. For the second horse, so far from dying in saving the first with its blood, remained quite unconcerned, so that it was clearly unnecessary to replace the whole of the blood that had been

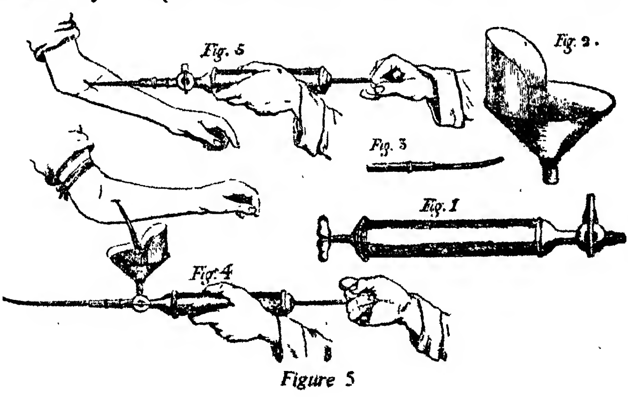

Figure 5

lost or anything approaching that quantity—a very important fact from the point of view of the human blood donor, who would not usually wish to risk his own life even if it were to save another. One of these experimenters, Dr. Charles Waller, himself practised blood transfusion to save women who bled in childbirth, and reported on instances of this in 1825 and 1826. He was writing about it again as late as 1859, and then illustrated his method with a woodcut, showing a funnel, a 2-oz. syringe lined with tin, and a two-way tap connecting them with a cannula entering the patient's vein (Fig. 5). His patients in 1825 and 1826 received 4 and 8½ oz. of blood respectively given with the syringe, and the lady whose life he claimed to have saved in 1859 received 8 oz. These quantities would now be regarded as too small in view of the condition of exsanguination for which they were given. Doubleday, in 1825, gave as much as 14 oz., which was bold, particularly as his patient remarked after she had received only 6 oz.: "I am as strong as a bull." The effect was indeed strikingly satisfactory, the patient's pulse-rate falling within a short time from 140 to 104.

Other operators during the nineteenth century were employing other methods of transfusion, and one of the chief exponents of a more direct or immediate manner of

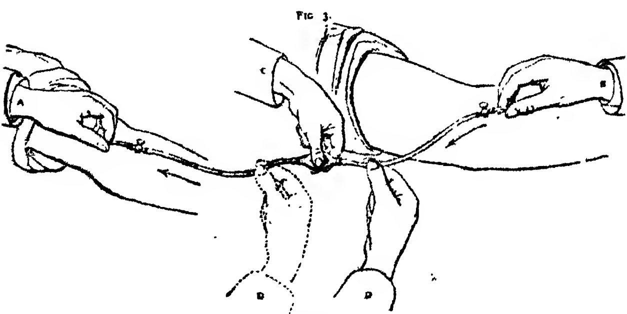

Figure 6

transferring the blood was Dr. J. H. Aveling, another obstetrician. He relates that he experimented in 1863 and as a result invented in 1873 the simple apparatus illustrated (Fig. 6). "It consists," he wrote, "of an indiarubber tube to form an anastomosis between the emittent and recipient veins and a little bulb in the middle to act as an auxiliary heart. With the exception of two silver tubes to enter the veins and two stop-cocks, this is the whole of the apparatus, and I carried it about in my pocket to every confinement I attended for eight years until at length the opportunity for using it arrived." This was in 1872 when he found a lady aged 21 *in extremis* from hæmorrhage after childbirth. She received 60 drachms of blood from her coachman, and

Figure 7

the incident is recorded in an admirable wood-cut (Fig. 7). She soon recovered enough to be able to remark that she was dying, though, Dr. Aveling added, "the mental improvement was not as marked and rapid as I anticipated, but this was perhaps due to the quantity of brandy she had taken." The coachman, he was pleased to record, was not only collected and cheerful, but able to make several useful suggestions during the process of transfusion.

Aveling, after describing what he believes to be the only

instances, seven in number, in which the direct method of transfusion had been used in this country up to 1873, claims as some of the principal arguments in its favour that the blood passing quickly through the tube is physiologically unchanged, and that "the chances of coagulation are small, because the blood is removed from the action of the living vessels for only a few seconds, and glides smoothly through the indiarubber tube without being exposed to the air."

All through the history of blood transfusion there was one main technical difficulty militating against the success of the operation, namely, the tendency of the blood to clot and so to block the tubes and other contrivances used in the process. It is probable that during the earlier transfusions of animals' blood some of the patients owed their lives to the formation of clots, for had there not been this technical obstacle it seems certain they would have received enough blood to kill them. As it was, a transfusion was to some extent a self-limiting process, and once the value of the transfusion of human blood as a life-saving measure had been established, attempts to make it easier by preventing the clotting were bound to follow. One of the first to try this was Braxton Hicks, an obstetrician of Guy's Hospital. He was advised that phosphate of sodium was an effective anticoagulant, and in 1869 he described a series of transfusions given with the help of this reagent. In his paper he first described attempts at transfusion in the old way. Thus in treating a woman for post-partum hæmorrhage he complains of various difficulties such as the faintness of the husband who was serving as donor. In addition to these complaints he goes on to say: "The apparatus employed was the funnel and syringe of Dr. Blundell [still in use after 45 years]. The coagulation of blood was the greatest trouble which interfered with the performance of the operation. The instrument had to be washed out three times, owing to coagulation during the check of the supply from faintness of the blood-giver, but I believe no clot was

injected into the vein." Accordingly in 1863–4 he performed four transfusions using blood mixed with one-fourth its volume of a solution of sodium phosphate; unfortunately all the patients died, and the poisonous properties of sodium phosphate may have contributed to this result. An attempt, however, had been made in the right direction.

Considerable use was also made of defibrinated blood, that is to say, blood from which the clot has been removed. This method was employed in 1835, and later by many others including, in 1873, Sir Thomas Smith who used it at St. Bartholomew's Hospital for transfusing an infant suffering from hæmorrhage of the newborn. His apparatus on this occasion included a wire egg-beater and a hair sieve with which to remove the clot. Defibrinated blood has been used up to recent times by various operators, but the process results in removal of a large part of the protein content of the blood and many of the cells, so that it is little better than a transfusion of serum besides adding much labour to the technique.

Although so much of the early work on blood transfusion had been done in England, and although its revival in the nineteenth century was started in England, yet most of the references to it up to 1874 are to be found in the works of Continental writers. Nevertheless, an important modification was introduced into the technique of the operation in 1857 by Higginson, who applied the principle of a rubber syringe with ball-valves for transferring the blood from the receptacle into which it was drawn to the vein of the patient. Higginson's syringe is now used for a different and less noble purpose, but it was successfully applied by its inventor in a series of seven transfusions which he duly reported. One patient was suffering from extreme weakness which was attributed to the too protracted suckling of twins. Higginson gave her about 12 oz. of blood from a healthy female servant, and a state of quietude followed her previous restlessness. A few minutes later the patient was seized with a rather severe rigor. It did not last long, but led to

a state of reaction and excitement in which she sang a hymn in a loud voice. The final result was good and Higginson reported that some benefit was obtained from five of the seven transfusions.

Throughout the nineteenth century the main use of transfusion was in the practice of obstetrics, and in 1873 an inquiry was carried out by the Obstetrical Society of London into its merits. The result does not seem to have been very encouraging, and transfusion was still regarded as a procedure that was only to be used as a last resource. Even at this date there was an attempt in Germany to revive the use of lamb's blood and a treatise on the subject was published by Hasse. Sentiment, if not science, seems to have suggested that there was something repulsive in bringing a lamb into the sick chamber and mixing animal with human blood, but it was remarked in a discussion on the subject that "it was only taking lamb in another form."

Science, however, was taking note and was disturbed by the number of severe reactions and even deaths that followed transfusions not only of animal's, but also of human, blood. These effects were for long attributed to the introduction of air-bubbles into the circulation in spite of Blundell's demonstration of their harmlessness years before. Finally, however, in 1901, the presence of agglutinins and iso-agglutinins in the blood was detected by Landsteiner and by Shattock working independently in Vienna and London. In 1907 the four blood groups were determined by Jansky in Norway, and the work was repeated by Moss at Baltimore in 1910. This was an advance of fundamental importance since it now became possible to eliminate most of the fatalities due to incompatibility of blood. A test could rapidly be made to determine the blood groups of both donor and recipient.

In the years following this important discovery various improvements were made in the technique of transfusion. Direct transfusion from artery to vein was re-introduced and often used, though a serious objection to the method

was the impossibility of knowing how much blood had really passed into the patient. Use was also made of the principle that clotting of blood is prevented or delayed by allowing it only to come into contact with surfaces coated with a smooth layer of paraffin wax. In 1913 the Kimpton-Brown paraffined vessel was introduced, and this was used extensively in the United States of America.

The final advance was made in 1914 when the use of sodium citrate as an anticoagulant was made possible by the work of Lewisohn in America and of several other workers in different countries, who all arrived independently at the same conclusion, thus again giving rise to rival claims of priority. It was proved that sodium citrate was at once effective as an anti-coagulant and also harmless in the quantity that was needed, and the first transfusion of citrated blood was performed by Professor Agote of Buenos Aires on November 14, 1914, a date which is, therefore, of the greatest importance in the history of transfusion. A method had at last been discovered which approached the ideal, since it united the four cardinal virtues of simplicity, certainty, safety, and efficiency. The "extraordinary success" predicted by Dr. Thomas Sprat in 1657 was now about to be realised, though there had been an unfortunate time-lag of 257 years.

Agricultural Front

Scope of Agriculture

As a natural science, agriculture overlaps with botany and zoology, physiology, chemistry, and other academic subjects, because essentially it deals with the proper feeding and the maintenance in health of animals and plants of economic importance, and the control of their lives in specialised directions for the production of fruit, milk, wool, etc. Thus the laboratory discovery of plant hormones finds practical application in the rooting of cuttings, the increase of fruit yields, the discovery of better weed-killers. Laboratory studies of vitamins and other food substances show the way to better feeding of livestock, and richer milk yields. The application of genetics in cross-breeding leads to new varieties of plants and animals able to withstand disease or inclement weather. Some of the more recent developments of this kind are discussed in this section of *Science News*.

But agriculture is also an industry, and this has always to be borne in mind. The introduction of a scientific method in market gardening and farming is not merely a matter of overcoming the farmer's traditionalism (which may have its germ of sense too), but also one of economics, and the cost of every change must be calculated. The aim of agriculture is to get a large yield of good quality for the minimum cost of starting materials and expenditure of labour, to get it not one year but every year, and to be able to choose the season of the year in which it is harvested. This is quite a different goal from that of the laboratory scientist, who rarely counts the cost. Various scientific "improvements" may be absolutely unworkable on a farm because the materials required are too expensive, or because the process requires a skilled technician, and farms have to be run with farm labourers. In industry complicated

processes can have specially trained men to guide them. On a farm this is impossible, because the work changes with the seasons, and a technical expert would be unemployed most of the year. "Agricultural Front" does not deal with these problems of farm economics and administration, but they are ever present in the background.

In another way, the practical nature of farming has influenced the development of a scientific agriculture. It has often been much simpler to kill a sick cow than to try and find out what is wrong with it, and as a result there is still an enormous field of ignorance in animal diseases, and animal physiology generally. Do cows sweat? Nobody has ever bothered to enquire, yet on the answer may depend the whole possibility of improving the wretched Indian native cattle by crossbreeding with English Jerseys. In the face of nature's whims, the farmer has developed a resignation and fatalism which the scientist is now trying to soften and make unnecessary. The general advance of science helps agriculture, and penicillin cures the cow's infection of the udder, as well as human diseases.

More Light on Potatoes

The war really brought the potato into its own. Chemists have analysed potatoes for vitamins, for protein (meat value) for fats: and not only old potatoes but new potatoes; not one variety but hundreds; not only the potato as a whole, but the different parts of one—the core, the cortex, the section just under the skin which the cook always peels off. Physiologists have been busy too: how much potato can a rat stand? will a young rat continue to grow on a potato diet? Pathologists have sought the reason why potatoes grown in England get persistently diseased, so that we have continually to import fresh seed potatoes from Scotland, where the potato remains healthy. Engineers have told us how to make a dehydrated powder of potatoes, which can be reconstituted with a little milk or water to give excellent mashed potatoes with no loss in food value.

Most important of all, the botanists have asked themselves whether we are growing the best potato possible: they remember that most of the potatoes grown in Europe are from a rather chance selection brought back from America by Sir Walter Raleigh—some hundreds of years ago now. So they sent a special expedition to the ancestral home of the potato, in South America, where most of its relatives were also most likely to be, and they brought back samples of as many different kinds as possible. Some of them were funny-looking things, rather like battered sausages, with purple skins and green insides: real wild potatoes.

Many of them have now been studied at Cambridge, with a view to seeing whether it would be worth while to breed from any of them. The chemists have analysed, the physiologists fed them, and the botanists have studied their agricultural properties. What sort of soil and weather suit them best; how much vegetative growth (leaves, etc.) does each plant produce; the time and quantity of flowering and seeding; the time of maturity; the degree of tuber production; the resistance to diseases, especially virus diseases; these are the kind of problems which interest the agricultural botanist. Many of the properties they investigate are inheritable in whole or in part, and can therefore be passed on by suitable cross-breeding, which can thus combine in one plant advantages possessed individually by three or four different varieties.

One property, in particular, on which they recently reported goes under the general heading of "Vernalisation." Quite a while ago the Russians discovered that the flowering and ripening times of cereals could be altered by giving the young seedlings an intensive period of light or heat treatment for some days. By applying this technique they were able to grow wheat further north than ever before. The short summer was no longer a bar, because pre-treated wheat would ripen quicker. The same has recently been found to be a property of the rice plant. If 7-day seedlings of rice are given a six weeks' course of

eight to ten hours daylight per day in the warmth, they grow up bigger and mature faster, even when the course is over, whereas under continuous illumination or the normal long summer day, and at low temperatures, their growth is slower and more prolonged. The controlling factor is exposure to short rhythmical periods of light.

In the case of the potato, 75 clones of 8 species of South American potatoes were compared, and the differences of reaction to light and heat were apparently inheritable. Some kinds gave no tubers if the plant was given long days of light, or if it was kept warm; other sorts were quite the reverse, and tuber-production benefited by just these conditions. It appears that the actual total light the potato seedling gets is more important than the lengths of "day" it is split into; a different situation from the case of the cereals.

How light influences plants and plant growth, not only at once but for months to come, is not known.

Trace Elements

A number of animal and plant diseases are now being traced to the presence or absence of particular minerals in the soil. It seems that in addition to iron for making blood, animals, and probably man too, require other minerals. It has been found that chickens need thirty to forty parts per million of manganese in their food, or they develop a disease known as Perosis which principally affects the bones: the legs are deformed and the wings unusually short. In Australia and some parts of Scotland, sheep become anaemic unless they are given doses of cobalt. One of the most recent discoveries on these lines is the investigation of Swayback in lambs. This disease is found in most counties of England and Wales, especially Derbyshire, where one in ten lambs born alive has it, in Australia and probably in South America, Sweden, India, and South Africa. It is a degeneration of the nervous system, the lambs are blind and unable to walk or stand, or at least

suffer from muscular weakness, and they usually get worse and die, so that the farmer can suffer serious financial loss. This is especially the case since the disease occurs sporadically, and as many as 90% of the lambs on one farm may be affected.

Investigation has shown that healthy mothers give birth to diseased lambs—sometimes to a mixed brood, some of the children being normal—and that the disease is not infectious. A mother who has borne an ill lamb can suckle a normal lamb without a bad effect. Neither is it a hereditary disease. If a diseased lamb can be got over the disease and survive to maturity, it produces healthy offspring.

So research swung back to the idea that there might be something wrong with the feeding of the sheep, and poisons were looked for. It was found that pasturelands in swayback areas often had a great deal of lead in the grass, "bellanded" as the farmers said, and the ewes were certainly getting plenty of lead into their bodies. On the other hand the sheep with the most lead often had normal lambs. Things might have stuck at this point for a long time if scientists had not grown aware of the importance of traces of metals in the food of animals and plants. They knew that cobalt is required in traces by sheep for normal health: perhaps some other mineral would cure swayback. Such is the case; provision of a copper sulphate lick prevents the appearance of the disease.

Exactly what the copper does is not apparent. It may be that the growing tissues of the lambs' nervous system need copper as part of their machinery of growing and functioning. Or it may be rather that the copper in some way affects the microbes which live in the young lambs' intestines—for another revolution in scientific thought is the realisation that the germs which normally live in our intestines and those of animals play a vital part, making vitamins and other special substances which the body needs. Not all germs can do these jobs, and if the wrong

sort grow they may be useless to the body, or worse.

What lends some support to this second interpretation of the effects of copper is new knowledge about a disease of cattle, an unpleasant diarrhœa, which occurs chiefly in areas of Somerset. It is thought that the disease is due to poisoning by a mineral, molybdenum, since the pasture (especially the clover) has a molybdenum content six times the normal. But dosing the cows daily for ten weeks with copper sulphate again prevents the disease. So it does look a little as though it may be the bacteria in the cows' gut which are really the sensitive agents in these mineral diseases, and that copper is a way of controlling them.

This is not always the case, though. Alkali disease, prevalent in the Middle Western States of America, is a disease of horses, cattle and pigs in which the hair falls out, the horn of the hoof sloughs, and the animals become listless and emaciated and often die. It has been traced to the presence of selenium in unusual amounts in the soil. Some, but not all, grasses pick it up, and the animals feed on the grass and get selenium into their bodies, where it behaves just like its close chemical relative, sulphur. Now sulphur is an important part of hair and skin and hoof, and selenium takes its place, as a very shoddy substitute. There is plenty of selenium to be found in the hair that falls out. Here, then, the mineral interferes directly in the animal's economy. The remedy has been to search for species of plants which would grow on seleniferous soil without picking up the toxic mineral.

Selenium apparently does not harm the plants themselves. But the presence or absence of other minerals in soil may be very important for plant growth. The most widespread instance of this is probably the need for boron (the main element in boracic acid) in one part per million or so. Without it plants are stunted, or develop brown dead areas in their tissues. The brown heads of cauliflowers may be due to this (see Plates 4 and 5), the brown heart in swedes, and so on. Copper is essential too, but more than a certain

amount is poisonous to the plant. A further catch in studies of this kind is that an essential mineral, such as manganese, may be present in the soil, yet somehow not available to the plants. A simple chemical estimation of the total manganese in a soil is thus misleading. From recent work it seems that soil bacteria may by their activities control the amount of manganese which is available, on the ration as it were, to plants. Little is yet known about soil bacteria, and this is one of the present spearheads of research.

Fresh Eggs and Ripe Bananas

Preserving eggs has for some time been a difficult business; freezing breaks the shells, and waterglass is a nuisance for bulk preservation, and not of high efficiency either. Recently the U.S. Department of Agriculture has been experimenting with a method of oiling the eggs. All the air is sucked out of the crannies of the shell, and then oil allowed to soak in its place. At the end, the egg has all the pores filled up with oil, yet it can be handled like an ordinary egg and does not require to be kept under oil or in a solution. Better understanding of preservative methods comes from the realisation that eggs go stale and bad not because of bacteria getting in and starting a fermentation, but as part of a natural process of ageing leading to the death of the living egg. The egg has a certain continuous metabolism: it is "breathing out" carbon dioxide just like the human and the hen but, unlike them, it requires no oxygen. If this carbon dioxide accumulates in the egg's neighbourhood, it slows down the metabolic reactions which produce it and so postpones the day of the egg's decay. Sealing up the shell has just this effect: carbon dioxide can no longer escape into the air, but collects inside the egg and acts thus as a preservative.

The same principle applies in the storage of apples. The fruit is alive after it has been picked from the tree, and continues to take in oxygen and give out carbon dioxide,

burning up the starch in its pulp. When all its starch reserve is exhausted, the apple dies and turns the rusty brown colour inside, which is also found in bruised fruit. This process leading to decay can be slowed down, however, as in the case of eggs, by an accumulation of carbon dioxide. The fruit is stored in the cold (but not frozen, which would kill it), and in the dark in an atmosphere containing ten per cent. of carbon dioxide. In this way it will keep twice as long.

Apples also produce another gas, relating to the natural gas from oil wells: ethylene. This gas has the property of making the apple ripen. As a result, if apples are stored close together in a quiet corner of a room, the ethylene they are all producing will accumulate and make them ripen quickly. If, however, they are spaced apart so that gas from one apple cannot affect another, and properly ventilated so that air carries the ethlyene away, they will ripen very slowly and keep well. As little as one part of ethylene in a million parts of air is sufficient to ripen apples, and an eight-ounce apple produces as much as one cubic centimetre of pure ethylene gas in its lifetime.

Ethylene will also ripen other fruit—pears, lemons and bananas. This is useful in the case of the banana, for instance, since the fruit can be picked green and unripe in the West Indies, and be ripened on arrival in England, so that it is perfectly fresh. The same gas is also useful for ripening wheat. Bread made from freshly threshed wheat is greenish, soggy, and poor in texture. The normal loaf is baked from wheat which has been stored for a month or so, but if the baker is in a hurry, he can treat his fresh wheat with air containing one part per thousand of ethylene, and a normal loaf is obtained straight away.

New Weed-killers

Weeds are important not only because they tend to crowd out the crop plant and steal its food, but because they may act as host to various infectious diseases of plants. The downy mildew (fungus) which attacks lettuce passes the

winter on thistles and wild sunflowers; the winter phase of the leaf-hopper insect, which infects sugar beet with "curly top" virus in the spring and summer, hibernates on thistles; so the complete elimination of thistles would remove these diseases. But the complete destruction of weeds is very difficult, for their root systems and seeds survive long in the soil in spite of chemical applications, and hand weeding is laborious and expensive, in fact one-third of the cost of producing onions and carrots goes on weeding.

Older methods of weed-killing aimed at destroying all vegetation with a suitable poison dusted over the ground—sodium arsenite, borax, or common salt, for instance. In time rain washed the soil clean and then the crop plant could be planted afresh in the hope of growing weed-free. To clean up one square rod of land in this way needed 250 lb. salt, or 15–30 lb. borax, or 3–4 lb. arsenite. Another way was to spray dilute sulphuric acid: the broad-leaved plants caught the full force of the spray, while the fine leaves of grasses, and plants with waxy coverings, escaped. One gallon of acid in 50 gallons of water was enough to spray 1½ acres of onions, and the cost was about a quarter that of hand weeding. Various oils were also tried in spray form. Paraffin is said to remove dandelions from lawns, and weeds from carrots and celery, but this technique is expensive. A related method which has been used successfully with two crops is a specially designed flame-thrower. This also is very expensive, since it needs a trained man to operate it, but of course it is quicker, and less of a strain on his back.

More recently various chemicals have been tried. One of the best known, "Sinox," the yellow dye sodium dinitro-orthocresylate, was developed in France in 1933. Its use illustrates the general principles on which weed-killers are based. For instance, it may be used on a field of mustard when the weeds are about 3 to 7 ins. high and have three to seven leaves. It can be used in fields of established transplanted onions until their shoots are 10 ins. high. It

must never be used on peas before they are 3 ins. high or after they have produced their first buds. Lucerne and red clover are killed, if they are younger than four to six weeks old. In other words the aim in weed-killers is to take advantage of the fact that some plants grow faster than others or may have started growing earlier in the season; and of the additional important fact that a plant is not equally sensitive to a poison throughout the whole of its life. In general, seedlings are much more sensitive than grown plants, and there are also various "physiological crises" of temporary increased susceptibility, such as the onset of flowering, during the full life cycle.

The latest development in the war on weeds comes in fact from closer study of plant physiology. In *Science News I*, Dr. Thimann described the plant growth hormones, chemical substances produced by the living cells of the plant at various times, which cause the formation of new roots, or leaves, and flowers and fruit. In common with many other laboratories, that of Imperial Chemical Industries at Jealotts Hill was interested about 1936 in experimenting with these newly discovered chemical substances, trying variants on the natural hormones, and testing the effect on the plant of different doses of these plant drugs. They found that whereas small amounts of hormones, such as 10 parts per million of α-naphthylacetic acid, were very beneficial to plants, increasing the dose to 100 or 1,000 parts per million was detrimental, the over-stimulation resulting in a short period of rapid abnormal growth, followed later by death. This effect was not found with all plants: thus putting down 25 lb. α-naphthylacetic acid per acre in a field of oats cleaned it of weeds such as yellow charlock, greater plantain, and yarrow, without harming the oats. Wheat, barley and rye could similarly be weeded chemically. The best time for putting the killer down in this case was found to be just after sowing. In such circumstances, 84% of the expected charlock never germinated at all!

Later work partly in conjunction with a Government sponsored body, the Agricultural Research Council, developed cheaper substances. 1 lb. per acre of 4 chloro-2-methyl-phenoxyacetic acid kills off all corn buttercups and marigolds, and all the poppies. 2.4.5. trichlorophenoxyacetic acid, in 500 parts per million, kills the perennial bindweed (convolvulus), on nursery fruit trees. It also, incidentally, will kill Irish potatoes. Much work has been done along similar lines in the United States, while the possibilities of these new substances are also being investigated in the tropics. The Institute of Tropical Agriculture, at Mayagüez, Puerto Rico, for instance, recently reported that sugar cane and coffee crops could be safely weeded with a 0.3% spray of 2.4 dichlorophenoxyacetic acid.

These new weed-killers are the cheapest and most powerful yet discovered. They can be sprayed or dusted on, they are non-corrosive and non-irritant, and harmless to stock. They are most effective on young plants, especially in periods of rapid growth on moist soil and well in the sun. Their discovery has come, not through a direct searching for better and better weed-killers, but as an offshoot of academic scientific work designed to satisfy the curiosity of scientists interested to know "how plants work." Fuller understanding of the natural chemical substances governing the plant's life has enabled us to turn their substances against it, to its own downfall.

Scientific Method and Philosophy

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CURRENT discussion of scientific method is largely concerned with the ancient questions of realism versus idealism, and causality versus indeterminism. Much less attention is given to the problem of scientific method itself: what can we find out by making and studying observations? When this problem is seriously tackled it is found that the two questions first mentioned can be stated in several forms, some of which can be definitely answered one way or the other, while others are demonstrably unanswerable by scientific methods and therefore have nothing to do with science.

A realist holds that there is a "universe existing independently of our thought and our examination of it," to use the words of Prof. Dingle. An idealist holds, on the contrary, that he is inventing a universe for himself as he goes along and that it has no existence apart from his thought. Most people are realists and many would consider idealism to be merely wrongheadedness. But when we try to examine the grounds for realism we find that it is extremely difficult even to say what we mean by "exist," and one modern school of logicians has decided that the problem is a pseudo-problem and is merely a matter of what language we choose to use. However, many people think that they know what they mean by it, and I am personally inclined to agree with them. But when we come to the more advanced problems of science we meet questions like "Does the electron exist, or is it merely a product of our imagination?"; and the idealist answer may be given by people who have no doubt whatever that

their chairs and tables do exist. The same people may reject all attempts to investigate the interior of the earth or the origin of the solar system as "speculation." They are realists in everyday affairs but idealists in relation to more advanced knowledge. Now this suggests the further question: "Is there any stage in the arguments that lead some people to assert the existence of the electron where a principle is used that is not used in the assertion of the existence of a chair? This leads us to consider why we believe in the existence of a chair. A man may say, "Well, I can see it and feel it." But without further analysis this begs the question. He has visual sensations of brown patches, which vary continuously in shape as he moves about; but none of these sensations is the chair. He may have also tactile sensations of pressure, but none of these is the chair either. In fact the chair is not itself experienced but inferred from sensations. The point is that by thinking of an object with a definite shape and size and suitably coloured (though the word *colour* applied to an object certainly does not mean the same as the same word applied to a visual sensation) one can predict an enormous number of sensations; and comparatively few sensations are enough to infer enough properties of the object to predict all the other sensations. The realist considers this a sufficient ground for asserting the existence of the chair, but there is no other observational ground for doing so. The idealist, however, would say that the chair is a mental construct, which he finds useful in classifying and predicting sensations. But he would attribute to this imaginary chair all the properties that the realist attributes to his supposed real chair, and would infer the associated sensations by exactly the same method and therefore predict the same sensations. Consequently there can be no observational means of deciding between realism and idealism: they differ only in regard to the indefinable notion of reality itself, and this notion, whether we accept it or not, is not used in the process of deriving properties of the object from

a few sensations and using them to predict new sensations. The fact that we cannot define it does not show that it is meaningless: a definition can only state the meaning of a word in terms of something more elementary, and there may not be anything more elementary. To most people reality does convey some meaning; but so long as a realist and an idealist confine themselves to the subject matter of science, that is, the making and discussion of observations, they will proceed in precisely the same way. We can believe in the reality of objects or not as we like; the decision will not make the slightest difference to science or to everyday life, though it may to philosophy.

Now if we consider the evidence for the electron we find that it is of just the same kind. The electron is not a sensation, but it does help to classify and predict sensations. The most direct evidence for its existence requires rather elaborate apparatus and careful experimental technique, but the essential principles are the same. Its properties are determined so that they will describe a number of actual observations and their consequences are then developed so as to predict other observations. There is one new feature: whereas the notion of a chair helps us only to predict sensations relating to things that are easily seen to be chairs, that of electrons also accounts for those relating to things that are not at all obviously electrons. It explains, for instance, why a lump of metal holds together, why it conducts electricity, and why its conductivity varies with temperature. But if this is a new principle it would appear that there is *better* reason for asserting the existence of electrons than that of chairs. But if we grant the realist position at all there is no ground for denying either. It is unreasonable to assert the existence of chairs and deny that of electrons on the ground that we can experience chairs and not electrons: we cannot experience either, but both help to explain experience. The chain of inference is longer for the electron, but the links are tested.

The attention of scientific workers was called to the

problem of realism versus idealism in the middle of the nineteenth century by Ernst Mach, an Austrian physicist who did important work in showing the experimental foundation of the laws of mechanics. Unfortunately he went astray in thinking that the problem was important for science, and he overlooked a really important one by use of the ambiguous term "description." We can describe experience that we have actually had; we can also describe experience that we expect to have, and experience that we might have if we were suitably situated. The first type involves no inference beyond the original data, the second does. The second type requires a new principle, that of *generalization*. The first serious attempt to understand it was made by Karl Pearson in the "Grammar of Science", which also contains some improvements on Mach's presentation of mechanics.

The need for an understanding of generalization is quite fundamental. It is not worth while to invent or postulate an object unless we find that we can co-ordinate many sensations by doing so. The Nautical Almanac's predictions of planetary positions, an engineer's estimate of the output of a new dynamo, and an agricultural statistician's advice to a farmer about the utility of a new fertilizer are all generalizations from experience. So are my expectations about the flavour of my next meal. Now if our reasoning is restricted to traditional logic such inferences are impossible. Traditional logic admits only three alternatives about any proposition: complete certainty, absolute denial, or blank ignorance. In pure mathematics it deals only with logical relations between nothing in particular, and is frankly admitted to have nothing to do with sensory experience until some extra hypotheses are supplied. In applied mathematics these are supplied, usually without mention of their origin, and results are deduced as exact consequences. Thus it is impossible to say anything at all until we can say it with certainty.

Generalizations from experience are not, however, made

with certainty. To say that they are involves the fallacy known in logic as the undistributed middle term. It appears in accounts of scientific method in various forms. Thus it may be said that a quantitative law is a description of the observations; but that refers only to the observations that have yet been made, and an infinite number of laws would fit any finite number of observations. To infer any new observation from it we must select a particular law from this infinite set; and without some rule not contemplated in traditional logic there is no means of making such a selection, or therefore of preferring any prediction to any other. Conversely, if we say that we learn our laws from experience we can avoid the fallacy of the undistributed middle only at the cost of admitting that there are valid inferences that are not made with certainty. We must, in fact, choose between three alternatives.

(1) Prediction is meaningless: then all practical men are wasting their time, the sun may rise in the west to-morrow, and human relations that depend on people understanding one another's language are impossible.

(2) Prediction is made deductively, not from experience, but from some general principle alleged to be logically certain; if experience plays any part at all it is only to fill in a few details. This is the attitude of many modern theoretical physicists. However, their general principles and their results differ even within the very limited field of knowledge where they have been applied; and at the most we can regard them only as guesses worthy of proper test by experiment.

(3) Generalizations from experience can be valid inferences, but are not made with certainty. But in that case traditional deductive logic is admitted to be inapplicable to either ordinary life or scientific method.

There are no other alternatives; nobody believes the first; the second is very doubtfully applicable at all, and has not been applied to the fundamental problems; the third is generally accepted, but asserts the inadequacy of

deductive logic. It demands the notion of a degree of reasonable confidence, of which certainty and impossibility are the extreme possible values; but the former, while it may be approached by a generalization or an inference from one, is never quite attained.

The need is indeed obvious without this discussion if we consider the kind of inferences that we actually draw. References to "exact science" suggest that physical measures are predictable exactly. This is not true. However carefully we try to repeat the conditions there is always a margin of variation unexplained. Everybody that has ever made observations is fully aware of these facts. Very few, however, have grasped the consequence, that any theory of inference that can make only exact predictions or none at all is for that reason inadequate. We must have a theory that deals with the probability of variations of different amounts. Such a theory was given by Laplace at the end of the eighteenth century. Heisenberg's uncertainty principle is of the first importance in physics, but the idea that it contains any startling novelty in philosophy can be attributed only to ignorance. Such problems are of everyday occurrence; but traditional logic has nothing whatever to say about them. That does not say that they cannot be treated mathematically; it is perfectly possible to generalise mathematics to give it greater elasticity, and modern physicists have done much in that direction. They have, however, shown a curious reluctance to recognise the inadequacy of traditional logic itself, being apparently saturated with the naive realistic ideas of applied mathematical teaching. Yet traditional logic denies the possibility of learning from experience, and it is right if we restrict this to mean learning with certainty; if we believe that we can learn from experience it must be with different degrees of probability.

The notion of probability as a statement of a degree of reasonable confidence goes back at least to James Bernoulli (1654–1705), and is quite explicit in the works of Bayes

(d. 1761), Laplace (1749–1827), and a number of other writers. Other definitions have been attempted, but they all lead either to the introduction of superfluous postulates or are insufficiently general to cover the ground. I mention only one. Some writers speak of "mathematical probability" as meaning simply that if there are n possible results of a trial and m of them include the event considered, the probability of the event is m/n. In earlier works, such as those of Laplace, this was stated with the proviso "provided that all the ways are equally likely." Thus it was not a definition of probability, since the notion occurred in the statement; it was a rule stating how to estimate probabilities in certain circumstances, the idea of probability itself being taken as already understood. If the proviso is dropped, however, we find that the "mathematical probability" of a coin coming down heads is 1/2 even if it has been loaded with lead on one side and has already given ten heads in succession. The definition is simply a concession to traditional logic and discards the essence of the matter; and traditional logic admits no compromise. It says that the result of a trial, or of any number of trials, is simply unknown and there is no more to be said.

The question is, then, can we construct a mathematical theory of probability, in the ordinary sense of the term, that will be consistent, and will meet the requirements of scientific procedure—and incidentally of ordinary life? It turns out that we can. The fundamental idea is simply that of the probability of a proposition, given the data; we need the postulate that such probabilities can be arranged in an order, and a few minor postulates and conventions that make it possible to assign numbers in a one-one correspondence to probabilities. The *principle of inverse probability* follows as a theorem. This considers the effect of new sets of data on the probability of a set of hypotheses, and states that each probability given the new data is proportional to the product of two factors. One factor

is the probability of the hypothesis given the previous data; the other is the probability of the new data given the hypothesis and the previous data. By taking new pieces of experimental evidence in turn we can always bring our probabilities up to date. The principle amounts to saying that the most probable hypothesis is the one that requires the observations to be the least remarkable coincidence. The results are consistent in the sense that if we compare the probabilities of two hypotheses at any stage of our knowledge, and other information is afterwards attained, the ratio of the final probabilities will be the same in whatever order the new pieces of information are taken into account. Inconsistency can arise if we speak of the probability of a proposition without reference to the data, which is still liable to be done on account of an inadequate notation that does not mention the data explicitly; but with a correct notation (due in principle to W. E. Johnson) no such trouble can arise. The essential point is that to say that we can learn by experience is the same as saying that the probability of a proposition is a function of both the proposition and the data. With this theory we can proceed to discuss what confidence we may attach to hypotheses, given observed data, if we are to follow consistent rules, and not to have different standards of validity depending, say, on whose hypothesis we are discussing.

Ordinary and scientific reasoning both admit learning from experience and therefore recognise the inadequacy of deductive logic; and everybody admits the significance of the statement "on data r, p is more probable than q." Different people may disagree in particular cases about which is the more probable, but they agree that the statement has a meaning. We start with the agreement and postpone consideration of the differences. It turns out that they can be attributed either to incomplete working out of the results (which can happen in pure mathematics) or to allowing our beliefs to be influenced by our wishes. Science has got on fairly well without any formal statement of its

principles, but it is definitely advantageous in practice to have such a statement, which will at least allow these complications to be placed where we can see them and may possibly enable them to be removed, since they are perfectly capable of being tested; and on the theoretical side it liberates inference from experience from the charge of being indistinguishable from a set of arbitrary assumptions. It turns out that the apparent assumptions are all closely connected and that the process is much more coherent than might have been thought.

One immediate result is that the traditional idea of causality must be discarded as possibly wrong and certainly useless. With the possible exception of mere counting, a scientific law is never exactly verified. This fact appears to be unknown to many philosophical critics and to be conveniently forgotten by most scientists when talking about the basis of science. We must either choose our laws to fit the data exactly or be satisfied with a compromise. In practice we always compromise. At the times when Euclid and Newton worked, the unexplained discrepancies were some hundreds of times those that ultimately led to modifications of their laws; but in spite of this the laws were taught as having deductive certainty. Scientific success does not consist in exact correspondence, but in the improvement of approximation. However carefully we measure, our measures never repeat themselves exactly; we may speak of "experimental error" in some metaphysical sense, but the solid fact is that the measures vary—unless we use so coarse a measuring scale that all variations are swamped by the step of the scale, and if we do that even such a law as the additive property of distance is not verified. Even the simple law that the length of a solid body is constant is not exactly verified.

If exact causality is relegated to the indefinite future as something that science may achieve some day, or into some unobservable realm underlying knowledge, it represents unwarranted optimism but possibly does little actual

harm. But it is disastrous to suppose that it has anything to do with the constructive development of science. It then demands the impossibility that we shall know everything relevant to the observations before we can say anything. It leads to the disparagement of careful attempts to synthesize a large number of data as "not final"; whereas anybody who has seriously tried to understand scientific method knows that no scientific advance is more than a step forward. The only practicable method is to start at the other end. We know that observations vary, and we ask ourselves whether the variation is wholly random or partly systematic.

Randomness is a technical term. In its most elementary form we say that a hand at bridge is random, or meant to be so, in the sense that out of the 52 cards that we might get, we are equally likely to get any set of 13, *whatever hands we have held previously*. The last condition is the essential one: it is achieved by thorough shuffling. The probability of getting a head when an unbiassed coin is thrown is 1/2 irrespective of the results of previous throws. (If there is a known bias there will still be a fixed probability of a head irrespective of the result of previous throws, but this will not be 1/2.) To take an example from genetics, if a man and a woman are both brown-eyed and each has a blue-eyed parent, then there is a probability 1/4 that any child they have will be blue-eyed, whatever the eye-colours of their other children may be. The common feature is that in each case there is a quantitative property of the system expressing the chance of a given type of event, and this chance is the same whatever the results of other trials. Such random series have a remarkable property discovered by James Bernoulli. If there are many trials, the event is practically certain to happen in a fraction of the trials nearly equal to the chance. In a hundred deals at bridge a person is likely to get the ace of spades about 25 times: the first hundred children of couples as described above that we meet are likely to include about 25 blue-eyed ones.

The matter can be put more precisely, but it is enough to say here that if N is the number of trials and x the chance of the event in one trial, then there is a probability of about 2/3 that the event will happen between $Nx \pm \sqrt{[Nx(1\text{-}x)]}$ times and one of about 19/20 that it will happen between $Nx \pm 2\sqrt{[Nx(1\text{-}x)]}$ times. In the above case we may say that, though the most likely number is 25, there would be nothing surprising in getting any number between 20 and 30, but it would be rather surprising if the number did not lie between 15 and 35.

We usually meet the inverse problem: we have good reason to believe that a chance exists but we do not know its value to start with. We make a large number of trials; then by the principle of inverse probability the most probable value of the chance is the fraction of the whole number of trials in which the event happens. This sampling ratio is our estimate of the chance. But since the same sampling ratio might easily arise from any value of the chance within a range, the chance is not uniquely determined. If we get, for instance, a sampling ratio of 0.30 from a sample of 100 trials, it might mean that the chance was anything from 0.25 to 0.35 with ordinary deviations from the most probable value given the amount of the chance. Consequently it is not sufficient just to give the most probable value; we should also give an estimate of the range such that there is a specified probability, given the observations, that the chance lies within it. Our estimate would then be written as 0.30 ± 0.05. The usual practice is to give the uncertainty as a *standard error*; the probability that the true value that we are trying to estimate lies within the limits indicated is about 2/3; errors more than twice the standard error are improbable and errors more than three times it are as nearly impossible as does not matter. This kind of problem has many instances, particularly in medicine. We may, for instance, want to know the chance of recovery from a disease under a particular treatment. The estimate will obviously be within narrower limits the

larger the sample, but the possible size of the sample is usually limited by some external condition.

The above problem is one of pure estimation. But it may happen that we have a suggested value of a chance, and are not sure whether it is right. In a genetical problem, for instance, we expect a chance to be 1/4, but there may be complications. Then if we take a sample of 100 and get anything from 20 to 30 of the type in question we shall regard the result as in accordance with theory and confirming it. But if we get 50 or 10 we can be confident that the suggested value of 1/4 is wrong for the problem in question. Such a case is one of *significance*. The unexpected result in such a case not only disposes of the hypothesis tested, but provides an estimate of the chance on a hypothesis substituted for it. The principle can be extended to test whether two chances are equal even if no special value is suggested for either by previous considerations. This case arises, for instance, in tests of inoculation. The animals used for the experiment are divided into two sets, and one set is inoculated. Both are then exposed to the infection to be considered. Usually some members of both sets catch the disease and some do not: if the difference of the sampling ratios is large enough to be significant the test gives evidence for or against the usefulness of the inoculation.

Incidentally the common remark "you can prove anything by statistics" is perfectly true, but not in the sense that its makers intend. It is quite possible, for instance, to find in an inoculation test that the inoculation makes no important difference, or that it makes matters worse. The point of experiment is not to prove a theory but to find out whether it is right or wrong, and the experiment must be arranged in such a way that analysis of the results can discover which. Many (perhaps most) important scientific advances have come from experimental results that their makers did *not* expect. But popular presentation of scientific results seldom gives enough information even to

provide a basis for discussion. The above type of test requires four numerical data before we can say what it means, but propagandists on either side often give only one or two. Such methods are either incompetent or dishonest. As an illustration of a method that will give the correct decision in the great majority of cases suppose that I plant 40 of A's scarlet runner seeds and 50 of B's. Of the former, 24 come up; of the latter 22. Are A's significantly better? We arrange the table as follows:

	Grew	*Did not grow*	*Total*
A	24	16	40
B	22	28	50
Total ..	46	44	90

More generally, if the four entries are

$$\begin{pmatrix} x, & y, \\ x' & y' \end{pmatrix}$$

we work out the expression, introduced by Pearson,

$$\chi^2 = \frac{(x+y+x'+y')\ (xy'-x'y)^2}{(x+y)\ (x+x')\ (x'+y')\ (y+y')}\ *$$

There is strong evidence for the existence of a difference if χ^2 is more than 8; if it is less than 3 and the smallest of the four factors in the denominator is more than about 100 there is strong evidence that any difference between the sampling ratios represents nothing but the ordinary uncertainty of sampling.

In the present case

$$\chi^2 = \frac{90\ (24\times28-16\times22)^2}{40\times50\times46\times44} = 2.3 \text{ nearly.}$$

Thus in spite of the apparent superiority of A's seeds the difference between the two sampling ratios 0.60 and 0.44 is no more than might easily have occurred by accident if the two sets of seeds had been taken from the same packet. It is very easy to underestimate the differences that can arise between different random samples from the same mixed class. Strong evidence for a difference would need

*χ^2 is read as *chi*-squared (*ch* as in German).

either twice as large a difference of the sampling ratios or four times as many seeds. As an example we take the following data from Greenwood and Yule on inoculation for typhoid.

	Attacked	*Not attacked*	*Total*
Inoculated ..	56	6,759	6,815
Not inoculated	272	11,396	11,668
Total ..	328	18,155	18,483

$$\text{We have } \chi^2 = \frac{18{,}483\ (56\times 11{,}396-6{,}759\times 272)^2}{328\times 18{,}155\times 6{,}815\times 11{,}668} = 56.2$$

This value is overwhelming, though on a casual inspection the data are not much more striking than those of our first example.

If we take the 2×2 tables

$$\begin{pmatrix}10 & 0\\ 0 & 10\end{pmatrix} \qquad \begin{pmatrix}9 & 1\\ 1 & 9\end{pmatrix}$$

we get respectively $\chi^2=16.0$ and $\chi^2=12.8$. In both cases there is strong evidence against equality of the chances; and granting that they are not equal the differences are clearly very large. Thus large differences can be detected even with small samples. It does not take many trials, for instance, to show that the kind of plant that comes up depends on the kind of seed that is sown. The existence of associations, complete as far as we know, is responsible for the notion of determinism, but they are simply an extreme case of randomness when the chance is nearly 0 or 1. They attract attention easily but it is a mistake to suppose that every natural phenomenon can be reduced to them.

Now in physics we habitually meet such complete associations. We observe, for instance, how the volume of the gas in an enclosure varies when we vary either the pressure or the temperature, keeping the other constant. It is found that reduction of pressure or increase of temperature always goes with increase of volume. Now suppose that for a given temperature we observe the volumes at 21 different pressures. Take as a standard pressure the

middle one, so that we have observations at 10 larger and 10 smaller pressures. We can then form a 2×2 table as follows:

Pressure—	*Volume*	*Less than standard*	*Greater than standard*
Less than standard	..	0	10
Greater than standard	..	10	0

It follows immediately that the chance of a large volume is not the same according as the pressure is large or small. Similarly we can study variation with temperature and show that the chance of a large volume depends on the temperature. That is as far as we can get by simple counting. But we have the new feature that the answer given by each measurement is not a simple "yes" or "no," and we have neglected much information by treating it as such. All the quantities are capable of taking values over a wide range. If the temperatures used, for instance, vary over a range of 100° and we read them to the nearest tenth of a degree, we have much more detailed information than is expressed by simply saying that a particular temperature is greater (or less) than 50°; similarly for the pressure and the volume. Now if p is the pressure, T the temperature, and v the volume, it is found that pv is nearly constant for given temperature, and its variation with temperature is nearly proportional to the change of temperature. In other words, we can choose two constants k and a so that pv is nearly equal to $k\ (a+T)$ for every observation. Now this is a very remarkable fact, even though we meet it first at an early stage of scientific training. The volume may vary by a factor of 4 in an elementary experiment; yet this rule predicts its value at any trial within perhaps 1 per cent. In accordance with the principle of inverse probability this is not to be regarded as a remarkable coincidence but as a general rule. It is not found that pv is exactly $k\ (a+t)$; but we call the discrepancies "errors" and regard them as themselves random. The usual method of determining k and a is to make the sum of the squares of the errors a

minimum. It is found that when the data are measures and not counts they can establish a significant connexion with far fewer observations. This is due fundamentally to the fact that the fit given by a quantitative law is far closer than counting attempts to describe. We may not know the law, to start with, but having found one that gives a close fit we assert it on the basis of the observations.

The two quantities k and a are called adjustable parameters: they are not known to start with, but the observations yield a probability distribution over their possible values. They are analogous to the unknown chance estimated in problems of sampling, and have application to new observations not included in the original ones. k actually depends on the mass of gas used; but a does not, and further a turns out to be the same for all gases. It therefore becomes convenient to combine it with T and call $a+T$ the absolute temperature. Now here we come to the point that by choosing a formula with sufficient adjustable constants in it we could fit any finite number of data, whether the formula has any application beyond the original data or not. An exact fit is no evidence whatever for the truth of a law if it contains as many adjustable constants as there are observations; for such a fit could be obtained for any such law, and we have no means of choosing between the laws. Evidence for a law does not exist unless the observations fitted are more numerous than the parameters, and then the fit is not exact. It is here that the notion of exact fit is definitely harmful to the understanding of scientific method. The actual question is, how many adjustable parameters can be satisfactorily determined from the data? They will be found with a specifiable uncertainty, except when they are as numerous as the data; but we can proceed by introducing them one at a time. To be accepted as significant the estimate of a new parameter expressing the change from the last formula tried must be somewhere between two and three times its standard error. The outstanding variation can then be

regarded as random. The essential feature of the method is that variation is regarded as random until there is positive evidence that part of it is not. The principle is incorporated into the theory of probability by the simplicity postulate, which says that just before we test any new parameter it is as likely to be zero as not. That is, we express no previous opinion about the need for change and leave it to the observations to say whether the suggested change is needed or not. It is necessary that the two hypotheses considered must be clearly stated as laws relating distributions of chance. The most important and often the most difficult part of scientific work is to ask the right question: once it is asked the gathering of suitable materials to answer it and their analysis to give the answer are usually matters of routine.

One unexpected result of the analysis concerns the probabilities of future inferences from laws that have been verified in many instances without need for modification having been shown. Such probabilities are found to approach certainty whether the law in question is true or not. What the result amounts to is that if the law is false, the true one differs so little from it that the two have given indistinguishable consequences hitherto, and therefore will probably continue to give indistinguishable consequences. Thus the adopted one will probably continue to give correct inferences with the uncertainty stated in it. The better the verification of a law, the harder it will be to think of an alternative that is not contradicted by facts already known to us. The theory therefore makes it possible to modify a law that has stood criticism for centuries without the need to suppose that its originator and his followers were useless blunderers. At the same time it gives an answer to the problem of "scientific caution"; this is best achieved by asserting no more parameters than are found significant, stating the results with the uncertainty found from the observations, and applying the law as far as possible beyond the original data (still allowing for the uncertainty

of its parameters) until a discrepancy is revealed, if ever. It admits the whole of human experience as valid data, and any clearly stated hypothesis is worth testing. If the tests are found to reveal significant differences between people, it is still possible to proceed to study these differences and compare them with other data; psychology is admitted as a legitimate science.

The theory also gives an answer to the problem of mistaken perception, which will be found discussed in almost every book on philosophy. We say that grass is green. But a philosopher will say that if we look at it through red glass it appears black and will discuss at considerable length how we can know which appearance to accept: he does not usually arrive at any useful answer, on account of the verbal confusion involved in supposing that "green" means the same thing whether it is applied to an object or to a sensation. Starting from sensations, we can classify them according to colour and shape. The characteristic shapes of grass leaves and tussocks are then found to be sometimes associated with green, sometimes with black. This is a fact of observation. It suggests the further investigation of other conditions associated with the respective colours, and tactile sensations then discover that the change is associated with the presence of the glass and recall to mind the fact that an obstruction usually does not merely change the colour of the visual sensation associated with an object, but obliterates both the colour and the shape. The colour of the object is then defined to mean the colour of the associated sensations when there is the least possible intervening obstruction. The approach by the classification of sensations treats all sensations as equally valid, but studies their differences: and the colour of an object is then seen to be a matter of the choice of the most convenient definition. The physicist gets much further by associating colour with the distribution of energy with wavelength, and finds that the difference of colour associated with red glass corresponds to the absence from the

transmitted light of the wave-lengths associated with the sensation of green. In this case the definition of the colour of an object found convenient from analysis of non-instrumental observations is also convenient for physics; it corresponds to the distribution of the energy in the light close to the object. Visual sensations through coloured glass are not rejected but put into another pigeon-hole, where they become important scientific data. Quantitative descriptions of the light of stars too faint to have their spectra studied in detail can, for instance, be made by photographing the stars through screens of different colours, and studying the intensities of the images.

The similarities between different gases and the laws of chemical combination given by Dalton (1808) suggested to Avogadro (1811) the hypothesis that gases have a molecular constitution, consisting of a large number of similar small bodies, such that equal volumes of every gas at the same temperature and pressure contain the same number of molecules. On the kinetic theory of gases these are in motion and frequently collide with one another and with the containing vessel. This group of hypotheses is found to explain not only Dalton's laws and the approximate relation found between pressure, temperature and volume in a given specimen of gas, but also some departures from this relation that are found when the density is comparable with that of the same substance in the liquid state. The latter lead to an estimate of the sizes of the molecules themselves. This estimate is an adjustable parameter and so far the argument shows only that the hypotheses are consistent with the facts considered. But when it is found that with the same value we can predict also the density of the substance in the solid state, and the thermal conductivity and viscosity of the gas, within a small margin attributable to the fact that the molecules of different gases have different shapes, the molecular hypothesis is confirmed strongly. This is why we say that molecules (and atoms) exist. The point to notice is that the hypothesis

explains several different laws relating to quite dissimilar phenomena. If a hypothesis merely explains one law it can tell us nothing that the law itself does not, and it is more likely to be untrue. But if it explains several it would again be a remarkable coincidence if the agreement was accidental, and the hypothesis itself can be considered confirmed.

The same feature can be illustrated at more elementary levels. We can recognise a table by sight without touching it, or by touch in the dark. But it is more conclusive to have both data; the visual table might be made of painted cardboard, and one known only by touch might be a high stool or a desk until we have felt it all over. In the discovery of Neptune the position of the planet was predicted from the previously unexplained perturbations of Uranus and afterwards verified by observation. Now it might have happened that the gravitational theory was right but the planet had too low a reflecting power to be seen. In that case astronomers would not have denied its existence; they would have continued to study the motion of Uranus and to improve the calculated position of Neptune in order to improve the predictions of the position of Uranus. On the other hand a new planet might have been found visually but might have had too small a mass to produce perceptible perturbations. Either type of evidence by itself would have extended knowledge of the solar system. But the two together are much better. The visual determination of the position was much the more accurate and led directly to a determination of the orbit of Neptune; but the perturbations of Uranus were still available and, given the orbit, gave a much better determination of the mass than could have been obtained without it. The common features of all these studies are the powerful confirmation of a hypothesis that arises when two or more quite different sets of data check it, and the derivation of knowledge by combining them that could be got, if at all, only with much more difficulty by studying either of them separately. It is sometimes argued that it is dangerous to apply a hypothesis

far beyond the original data; but that is just why we get the best possible confirmation of the hypothesis if the prediction from it succeeds, and the clearest indication of how the hypothesis should be modified if it does not.

SUGGESTED READING

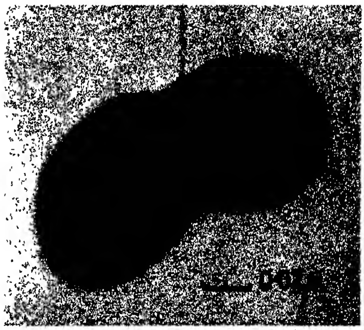
Karl Pearson, *The Grammar of Science*, 1892; reprinted in Everyman's Library (general account of scientific method).

Jeffreys, *Scientific Interference*, 1937. (Theory of measurement, leading up to observational presentation of the theory of relativity.)

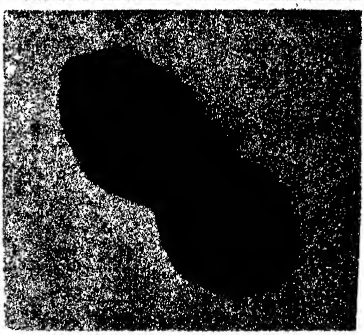
Jeffreys, *Theory of Probability*, 1939.

Rayleigh, *J J. Thomson*, 1942 (contains an elementary account of the evidence for the electron).

1. Synthetic Emeralds.
(see page 22)



2. Electron microscope picture of two individuals of the bacterium *streptococcus faecalis* (x 36000).



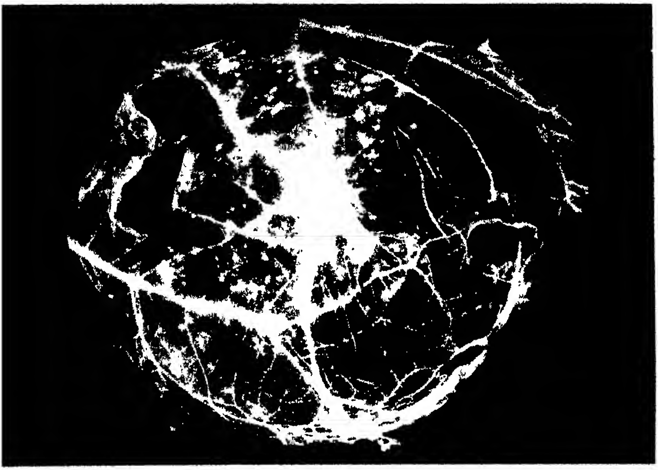
3. The same, after treatment with the antibiotic Tyrocidin (see page 90). Note how the outer layers of the organisms have been stripped off and collected in the groove between the two individuals (x 36000).

4. **Deficiency** of Boron in the cauliflower causes browning and rotting of the white head, and . . .

5. . . . rotting of the inner part of the stem, here seen cut open (see page 51).

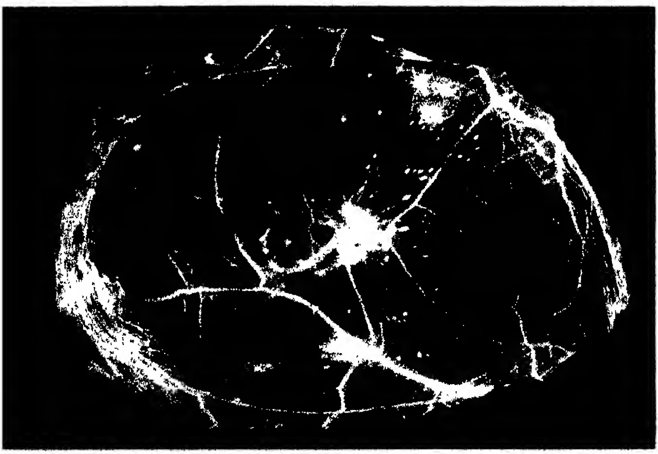
6. Blood Transfusion: Purmann, 1705.

7. Blood Transfusion: Elsholtz's figures, 1667.



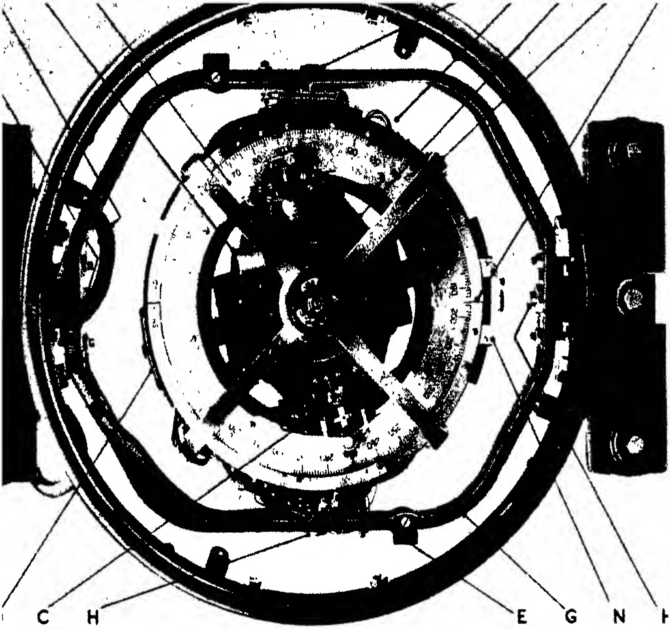
8. Influenza. Virus infection of egg membrane. Provides one method of propagating a virus strain after its isolation from the human patient.

9. Influenza. Virus infection of egg membrane. Discreet " pocks " are produced by a small dose and can be counted to estimate the strength of the original virus preparation.

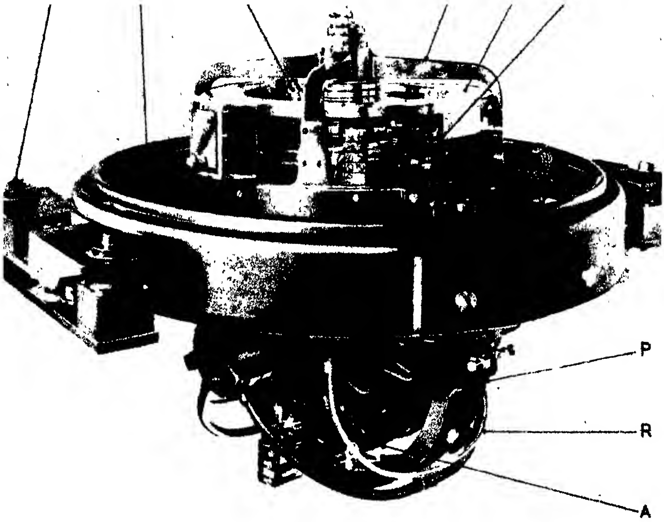


10. Nasal passages of a normal healthy ferret.

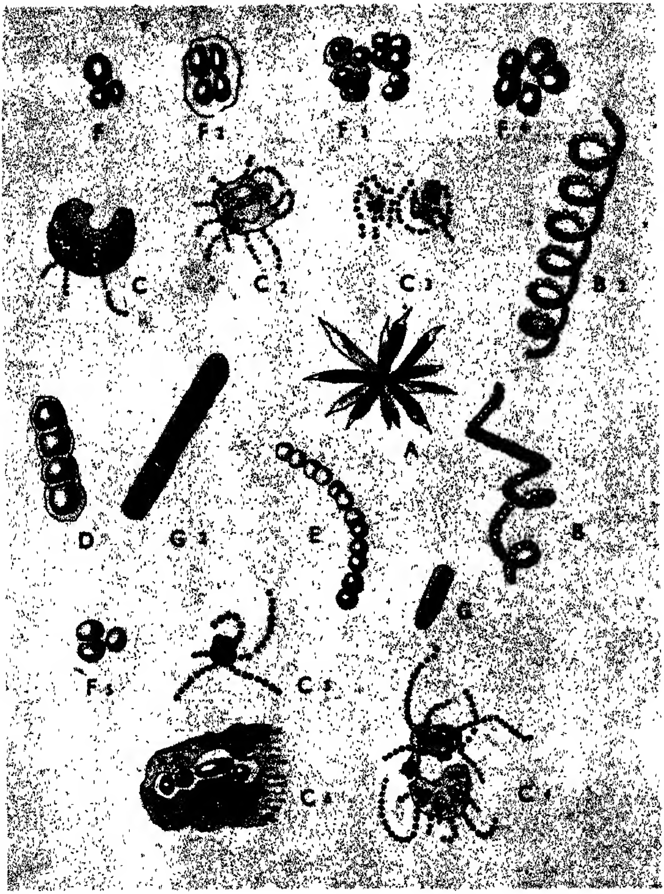
11. Nasal passages of ferret infected with influenza.



12. The master gyro-compass of the type used in the Royal Navy seen from above. This master transmits the direction of the ship's head to repeaters wherever they may be required in the ship. A, case containing the rotor which provides the directive force; B, gauge to indicate the vacuum in the rotor case; C, the contactor which passes a signal when the phantom is out of line with the rotor case; D, the Azimuth motor which drives the phantom after the rotor case; E, transmitters which are driven by the phantom and which send out impulses to the repeaters; E, the Spider which supports the moving parts; G, gimbal ring to maintain the compass with its card horizontal; H, gimbal pivots; J, shock absorbers to support the compass and protect it from the vibration of the ship; K, cable carrying electrical supply to and from the master; L, card; M, lubber line; N, corrector mechanism for making adjustments for speed and latitude.



13. The master gyro-compass of the type used in the Royal Navy. A, case containing the rotor which provides the directive force; C, the contactor which passes a signal when the phantom is out of line with the rotor case; E, transmitter which is driven by the phantom and which sends out impulses to the repeater; F, the Spider which supports the moving parts; G, gimbal ring to maintain the compass with its card horizontal; J, shock absorbers to support the compass and to protect it from the vibration of the ship; L, card; P, phantom which follows the rotor case; R, pipe connecting mercury pots which control the rotor making it North-seeking.



14. IODOPHILE MICROFLORA OF CATTLE (*highly magnified*). A, rosette association of unidentified organisms; B, giant spirillum; C—C_5 stages in disintegration of starch grains by coccoids; C_4, surface of grain showing lacunæ surrounding coccoids; D, coccoid chains (giant form); E, coccoid chains (normal type); F—F_3, sarcina packets; G G_2, *Oscillospira guillermondi*.

Medical Front

Old Age and Arrow Poison

ONCE upon a time surgeons used to tell one another a fairy story that is was impossible to operate on old people without killing them in the process. An old man with high blood pressure and a bit of bronchitis was not a good surgical risk. So, the tale went, if something awful happens inside him do not open him up to try to put it right but make him comfortable and let him die in peace.

We know now that this is wrong. The great danger with old people is that they easily get pneumonia, particularly after an anæsthetic, breathing strange and sometimes irritant gases as they do. To avoid this danger needs good nursing and a good anæsthetic, and given these two requirements, surgery on old people becomes more satisfactory, and often a successful life-saving measure. Now the anæsthetic has two functions. From the patient's point of view it makes the operation painless and helps him to lie still. And, just as important, it helps the surgeon by relaxing the patient's muscles. The abdominal muscles must be quite slack before an abdominal operation is feasible, and normally this means quite deep anæsthesia; and the deeper the anæsthesia, the more likelihood of chest complications afterwards.

The scene now flashes to South America, where the Indians use a famous arrow poison, a trace of which paralyses the victim pricked by an arrow. This arrow poison has been brought to the laboratory, analysed, tested on animals. It is not an anæsthetic, but it paralyses muscles by preventing nerve messages from the brain reaching them. The muscles lie slack and idle, but the victim may be fully conscious yet unable to cry out or move, a prisoner in his own body. A large dose of tubocurarine, as it is called, kills by paralysing the breathing;

smaller graded doses merely slacken the abdominal muscles, stop the speech but leave the breathing, coughing and swallowing intact. Using it in anæsthesia (a very skilled matter obviously), we get the surgeon satisfied by the slack muscles, and then the patient needs only a very light anæsthetic, a whiff of gas, to make the operation painless. The need for deep anæsthesia with its complications, is avoided. It is just a hundred years ago since the first anæsthetic of any kind was given, and now the anæsthetist has a very wide range of specialised techniques in his cupboard.

Insect War

The last few years have seen great success in fighting the nuisance and in some cases danger of insect pests. In DDT we now have one lethal weapon. In the less publicised but equally effective Gammexane we have another. This substance, chemically a benzene hexachloride (benzene from coal tar, treated with chlorine as discovered by Faraday in 1825), can be used as a dusting powder or spray, keeps well, is no more toxic to man than DDT and is powerfully active against most insects, including mosquitoes, locusts and weevils, and also bees. Another aromatic compound, benzoate, is highly effective against the itch-mite which burrows beneath the skin in the disease Scabies. So the list of insect poisons grows.

A different method of attack is the spreading of insect diseases caused by bacteria. Thus Japanese beetle in the United States is fought by inoculating the soil with powder of the spores of Bacillus Popilliae which infect and kill it. Very little is known about insect diseases, so this method is still in its infancy, though the use of insect parasites is, of course, widely practised.

Alongside these offensive measures research has also been developing passive defence, by trying to find out how insects seek out and bite their human prey. If we knew this method we might be able to put them off the scent.

Strangely enough human smell, at least the gross sort termed "body odour" in the advertisements, does not seem to be the call sign for insect recognition. People thoroughly soaked in deodorants are still found and bitten by flies on as large a scale as those who have taken no precautions. Two other suggestions have been made: that insects recognise that the atmosphere is warmer close to a living body and snuggle up to it; or that mosquitoes, for instance, recognise the carbon dioxide or some other exhalation (e.g., the moisture of the sweat from the skin), and move in that direction. Research is hampered by the fact that individuals vary greatly in attractiveness, and the causes of this variability have not yet been traced.

A simpler approach has, however, brought the beginnings of success—the search for smells that insects do not like. The most effective of these to date is dimethylphthalate, sold in the chemist shop as DMP. This stuff makes the most attractive human repulsive to mosquitoes, flies and midges, chiggers, ticks, for about six hours when applied directly to the skin as a 5% cream, and it can also be used to impregnate cotton underclothing and so keep that free for a time too. For country picnics in a temperate climate, this result is probably good enough, but of course the real importance of insect repellents is for tropical countries, the Burmese jungle and the malarial swamps of South America, where a six-hour respite means little. On their behalf the search for better bad smells continues.

Artificial Insemination

An article by Dr. Kenneth Walker and his collaborators in the medical press describes some of their experiences with the new techniques of human artificial insemination, a practice which is growing rapidly in importance following the success of similar methods in agriculture. It has been realised for some time that childless marriages in which both parties desire offspring may be due to faults in either husband or wife equally, and while a little is now known

about female sterility and something can often be done to cure it, the male has been much less investigated and is proving a more difficult problem. This is because a man who is impotent is unable to procreate, however good the quality of his semen, whereas frigidity in women is reproductively unimportant and impotence and frigidity being often due to psychological factors may be very difficult to treat. Doctors, however, now offer two solutions for childless couples in which the woman is proved normal and the fault lies with the male. If he is impotent but his semen is normal then artificial insemination using the husband's semen is performed. If, however, the semen is of poor quality, or contains genetically undesirable characters (i.e., an inheritable factor leading to deafness), then artificial insemination with semen donated by a third party is available.

This second possibility is at first sight revolutionary in nature. A wife who has a child by a man not her husband has always been frowned upon, and the law has consequently something to say on the matter, yet in the case of artificial insemination by donation, the father is not the husband but a man completely unknown to the mother. This is really quite a logical extension of a process of social change which has been going on for some time. It is now widely accepted that love and reproduction are two human activities which can be largely separated—witness the prevalence of contraception and so artificial insemination becomes a conceivable procedure. At any rate it is so regarded by a growing number of childless couples, and as Dr. Walker emphasises, so long as the technique is employed with discretion, it is unlikely to do social harm and will often do good.

What does discretion mean here? It means explaining to both partners of the marriage exactly what is involved: that no likeness of the child to the father can be guaranteed; that, in fact, conception may fail in spite of numerous inseminations; that there are legal difficulties; that the

attitude of the "father" to the child may subsequently change for the worse; in short that artificial insemination from a donor is a serious matter which demands careful consideration by both husband and wife before it is undertaken. On the doctor's side it means a very careful choice of donor, who must be at all times completely unknown to the recipient. Dr. Walker favours normal, healthy, intelligent men between thirty and forty-five years old, married, with at least two children already, and with the same blood group as the mother or the "father"—to avoid subsequent proof of non-paternity by blood-group tests on parents and child. He calculates that since as little as 0.01 cc. semen (a five-hundredth of a teaspoonful) is enough for procreation, and a donor can provide two volumes weekly, he could beget 400 children weekly, or 20,000 a year. This would inevitably lead to a number of half-brother and half-sister marriages twenty years later, which would probably be genetically very bad, so an arbitrary limit of 100 offspring per donor has been set. The usual practice is to perform twenty inseminations with each semen sample, by means of a special syringe, the wife attending the clinic on the day of the month on which her temperature on waking in the morning is at its monthly lowest, since this is the most favourable day for conception.

So far, here and in America, donor insemination has been practised in about 3,000 cases, with considerable success, and experience is rapidly widening. It marks a new step in man's control over the processes of his own body, and brings the practical possibility of Mr. Aldous Huxley's Brave New World a stage nearer.

Ill Before Birth

In 1940, Australia had an epidemic of an unusually severe form of German measles. In the following year doctors were struck by the unusual number of babies they were seeing who were born blind. This suggested to Gregg the possibility that a number of pregnant women had had

German measles and that the infection had spread to the unborn child within them and partially arrested its development. Thus it came about that an unusually large number of babies were born blind or deaf or with congenital heart disease.

To prove the correctness of this idea is not easy. Whenever he met a blind or deaf child Gregg asked the mother if she had had German measles during the pregnancy; and if so, what part of it. Most of them admitted that they had been ill during the first two months and were told then that this fever was probably German measles. But sometimes they did not remember very clearly, and perhaps it was some other disease, scarlet fever for instance, which they had. Women who had ordinary measles produced normal babies, that was definite. On the other hand it is just in the first two months of its life that a baby's eyes and heart and ears are appearing: thereafter it devotes its energies to growing larger; so that an illness would probably upset it most in the first two months.

Another way of trying to prove a connection between German measles and a bodily defect is by seeing what sort of baby is produced by every pregnant woman who has had the illness. A beginning has been made on these lines, so far with disappointing results. Of seventeen mothers who had German measles during pregnancy, for instance, every one produced a normal baby. So there is no inevitable connection between measles and blindness, and the investigation continues.

We know very little, except in the most general way, of the influence of the mother on the development of her unborn child. Certainly an ill-fed mother is more likely to have a miscarriage and the poor, who cannot afford good food, have more premature babies than the rich. But of detail, there is nothing in medicine to parallel the experiments on pigs. Sows kept for a long time on special feeds containing no vitamin A give birth to completely blind (and often eyeless) piglets, which may show other abnor-

malities as well, such as cleft palate or hare lip. But the cause of human abnormalities of this type is not known. If further research supports Gregg's idea, it will be the first step forward in elucidating the faults of the baby's development; and we may then find a mixture of nutritional and infective causes—perhaps that it needs not only the infection (German measles) but also an undernourished infant (the mother lacking vitamins?) acting with it to produce congenital disease.

Help for the Early Child

Nobody knows why it is that most babies are born into the world nine months after their conception. There is no sudden change in the child's development at that time. It grows smoothly and continuously month after month with no sudden breaks, and at nine months it still has a long way to go. In fact at that age it is not particularly well equipped to face the outside life; but at a pinch it can often manage it at an even earlier age, and some try to. There must be some kind of alarm clock inside the mother which decides the moment of birth, but it still remains to be discovered, so why some infants are born early at eight or seven months is a mystery. It is also a misfortune because the premature infant may be hard to keep alive. It is difficult to feed, prone to vomit or develop diarrhœa, and waste away and die.

A new scheme, developed in Sweden, America and Britain, seems to offer fresh hope for the unexpectedly early baby. Milk is a food difficult for it to digest, and the new idea is to provide it with something simpler to eat, but based on modern knowledge of nutrition. Milk consists of vitamins and salt, sugar, protein and fat all mixed together in about 80% of water. Fat is known to be relatively unimportant, and can therefore be missed out of the simplified food, but it is found that protein causes difficulty. Even the normal baby is not well fitted to digest protein, because its stomach does not make the essential hydro-

chloric acid until it is nearly a year old. The premature infant is in a worse case, and probably has not got a full set of digestive enzymes either (though we have as yet little knowledge on this point). So in the simpler food we have pre-digested milk protein, casein ready broken down into its constituent amino-acids. Feeding tests with this ready digested protein and sugar have given excellent results; the premature babies no longer get diarrhœa and vomiting so easily, and waste away and die; but they grow fast and turn into normal children. The fact that infants have an inadequate digestive system also, incidentally, gives them a slight advantage over the adult when it comes to penicillin treatment. They need not undergo the frequent intra-muscular injections which are sometimes painful. Penicillin is not destroyed in their stomachs and can be given successfully in their feeds.

Why didn't somebody try this predigested milk idea before? No doubt the idea has been floating about in people's heads for a good many years, but making it a practical fact depends on a number of new developments. In the first place, it is only in the last ten or fifteen years that the university chemists have learned the technique of digesting protein in the laboratory instead of in the stomach, for before it was not easy to digest it to the stage the body likes without spoiling it. It is only in the last ten or fifteen years also that doctors have realised how badly off the newborn child is. Its kidneys, for instance, unlike the adult's, cannot adjust their output to the water input. Even though it is kept thirsty, a baby will continue to make urine, and so dry itself up inside; and on the other hand, if it drinks largely, its kidneys fail to get rid of the excess fluid by making urine, and so it becomes water-logged. In other words, the niceties of infant nutrition are only just being realised.

Vitamin News

Most people are only at the stage of learning that there is

more than one vitamin B, and that we distinguish them as B1, B2, and so on. In fact scientists have been racing ahead, discovering so many vitamins that they have taken to giving them names instead of numbers. Originally, twenty or more years ago, they thought there was only one substance needed in minute quantity for growth and health which would dissolve in water, and they termed it "B." Now they have a list of water-soluble "Bs" running into double figures—pantothenic acid, biotin, p-aminobenzoic acid, inositol, pyridoxine, and so on. Some are known to be important for man: others, as yet, are only vital to certain animals.

The latest discovery is folic acid, so-called since it was first detected in the "foliage" of spinach, later also in grass, mushrooms, yeast and liver. Now that its chemistry is known (the chemists call it pteroylglutamic acid and find it is related to the colour in butterfly wings) it turns out to be the same stuff as a number of other vitamins. Vitamin M prevents anæmia and diarrhœa in monkeys. Vitamin Bc prevents anæmia in chickens. Vitamins B10 and B11 are needed by chicks for growth and the production of feathers. But all these vitamins are one and the same, folic acid. Different investigators have used different experimental animals, thought they were tracking down different vitamins and in the end have reached the same terminus. It makes the reports on vitamins very confusing to follow, but gradually order grows amidst the chaos.

Often with vitamins one finds that some animals can make them for themselves, others must be given them in their food. Rats, for instance, can get along without folic acid in their food, but humans probably cannot. This was an accidental discovery. Animals lacking folic acid often have few white cells in their blood to fight infection, so some folic acid was given to hospital patients with the thought that they might be deficient in the vitamin and that it would pep their white cells too. It did not do this, but one of the patients who also had pernicious anæmia

suddenly got better. It turns out that people with pernicious anæmia can be cured and kept well on folic acid tablets by mouth. Previously they have had to have injections of liver extract, and before 1926, when the liver treatment was discovered, they usually died. A tablet of a pure chemical substance is better than an extract of an animal's liver; it is also pleasanter to take: and in twenty years medicine has struggled a little closer to its ideal.

Collaborators

So the new knowledge about vitamins is that there are many of these chemicals required in traces in a normal healthy dietary. But another important recent discovery is that the whole vitamin business is more complicated than the nutrition scientists first thought. The original idea was, miss a vitamin and you got a disease. Take the vitamin, and get better. Thus food without vitamin C leads to scurvy, which gets better if fresh fruit (containing vitamin C) is given. Lack of B1 leads to a neuritis, and to beri-beri, which gets better if the vitamin is fed. This view was supported by experiments on animals and by observations on people who had been poor and ill-nourished for years. But when finally experiments were done on human volunteers, a different picture appeared. Nine people were kept on special diet lacking all vitamin B1. Every day their urine and faeces were analysed for B1 to see how long they would go on excreting it when they no longer received any, and how soon after that ill-health developed. After five weeks four of the nine volunteers showed signs of illness and their excretion of the vitamin had dropped almost to zero. But the other five kept obstinately healthy and continued to excrete vitamin although they were not getting any in their food.

This result was at first shattering. Where was the B1 coming from? Did it mean that people differed enormously amongst themselves and some bodies could make vitamins but others not? The experimenters saw another

possibility to consider first. They knew that everyone has harmless bacteria living in the intestines, particularly the large bowel, and bacteria in general are much better at chemistry than we are. They can live on the most indigestible foods, and make the most amazing chemical substances. It seemed possible therefore that under certain circumstances they might be making vitamin B1 and handing it over to their human hosts. Many of them, like disease germs, are killed by sulpha drugs (M and B for instance). So the five volunteers were given sulpha drugs, and at once their excretion of vitamin B1 dropped. The mystery was explained: some, but not all people have micro-organisms inside them to make vitamins for them.

The same is true of rats. It was noted on an earlier page that rats do not need folic acid in their food; but if they are dosed with sulpha-suxidine, which kills off their bacteria, they begin to require it. The rat cannot make folic acid for itself, it normally relies on bacterial workers for its supply.

So now attention focuses on the bacteria which inhabit the intestine. How useful are they? What can they do? Do they contain the answers to mysterious diseases? There is further mention of them on other pages, for the recognition of our unseen collaborators is a recent and important step.

Chilblains

Amongst the remedies used for chilblains in the A.T.S. during the war were applications of whisky, raw potato, roasted onion, snow, and urine, and drinks of hay tea and colloidal iodine. The actual full list of remedies tried by different people is too long to give here—these were some of the more exotic, reported in the *Lancet*, as the result of a survey of 3,000 members of the Auxiliary Territorial Service made to find out how important chilblains really are. As they are not deadly, or a cause for stopping work, science has not hitherto spent much time on them, and it

must be admitted that up to date the right treatment has not been discovered, and even the cause of chilblains remains unknown.

From the *Lancet* report it seems that every other woman will have had chilblains by the age of forty. So the illness is extraordinarily common. They are most and commonest amongst indoor workers, typists got them mostly on their hands, storekeepers and other people on their feet; a great many got them chiefly on their toes. People working out-of-doors, for instance manning guns, were much less liable to get chilblains than those in buildings. An interesting point is that in the U.S. and Canada where central heating is common, chilblains are rare. It looks as though the unevenly and badly warmed rooms so common in Britain may be to blame. Don't stagnate in front of a fire or in a draught if you want to escape the curse. But as for treating the curse when it comes—science is baffled.

Antibiotics

Many moulds and bacteria, and other plants, produce substances which are active in killing disease bacteria. Of these antibiotics, penicillin is the best known, and far and away the most effective. Commonly the antibiotics are too unstable for medical use, or toxic for man as well as the germs, or not powerful enough. However, with the success of penicillin the value of one or two others is being explored further. The most hopeful to date is one called streptomycin, chemically quite distinct from penicillin, and made by a totally different kind of mould. It was tested first on bacteria growing in culture in the laboratory and raised high hopes of its value in medicine, since in this test it is effectively germicidal against a number of bacteria unaffected by penicillin: typhoid, food poisoning organisms, plague, whooping-cough, and most exciting of all, tuberculosis.

A sober report from the Mayo clinic in autumn, 1946, however, reveals a good many of the difficulties and disappointments. One is that the stuff as at present available

does produce some harmful effects on many patients: sickness, fever, skin symptoms for instance. Another is that the bacteria very quickly get resistant to the drug: they are either killed straight away or they become indifferent to streptomycin and flourish (there is not the same difficulty with penicillin). A further point is that the drug is expensive, difficult to make and in short supply, and consequently likely to be used in insufficient dosage. It is only now that penicillin is freely available that enormous doses can be given, and further diseases brought under control. Infection of the valves of the heart, for instance (bacterial endocarditis) is now being treated with a million units of penicillin daily for a month. This is a great contrast to the usual course of 250,000 units a day for three or four days, which is useless for this particular disease.

Summarising the effect of streptomycin on 54 selected cases of tuberculosis, the authors of the report say: "Evidence clearly indicates that streptomycin does not exert a rapidly curative effect on clinical tuberculosis although it does appear to modify the course of the disease in a favourable manner in some cases."

Streptomycin can be given by mouth, but it is then not absorbed into the body. It passes through unchanged (penicillin is destroyed) and may therefore prove valuable in the treatment of intestinal infections. It is otherwise given as a three-hourly injection of about one to three grammes of material daily, with promising results in tularaemia, a disease common in America, and in certain types of meningitis in young children. Research is still in the very early stages and we may have better news at a later date. How streptomycin and penicillin act on the germ is not known ; they cause no immediate visible changes. Only in the case of another antibiotic, unfortunately too poisonous to man to be useful in disease, Tyrocidine, is the mode of action known. It behaves like a very strong soap, and washes off the outer protective layers of the germ. (See Plates 2 and 3).

Colour Photography has arrived

JACK H. COOTE, F.R.P.S.

For the vast majority of photographers, colour photography will have "arrived" only when it is possible for them to load a camera with film which, after exposure, can be sent back for development with instructions to "make a colour print from all the good negatives." The amateur can do just that in America to-day—in fact so many of them wish to, that the Eastman Kodak Co., who will have to do the printing and processing, are erecting a new colour laboratory in which they will be able to make *one hundred million* of these colour snapshots each year.

This achievement—perhaps the greatest since the beginning of photography—was only realised after a hundred years of searching, the course of which can be traced back to the work of Clerk Maxwell during the last century.

Early History

Nine out of ten textbooks dealing with the subject will tell you that Clerk Maxwell was the first person to put Thomas Young's three-colour theory of vision to practical photographic application. But if we take the trouble to look up the report of Maxwell's demonstrator, Thomas Sutton, we shall find that although Maxwell clearly outlined proposals for photographing and reproducing a scene in colour by means of three primary colours, in his practical demonstration he actually employed four different negatives—red, yellow, green and blue. No one seems to have discovered why he departed from the theoretical three colours, or for what purpose the yellow record was made.

In order to obtain his colour photograph, Maxwell made

positive transparencies from the four negatives, and images of these were projected from four lanterns fitted with filters corresponding to the taking colours—and, says Sutton, "when these different coloured images were superimposed upon a screen, a sort of photograph of the striped ribbon was produced in natural colours." This system of colour synthesis represents the *additive* method of colour reproduction, since coloured rays of varying intensity and proportion are added together to illuminate an otherwise darkened screen (Fig. 8).

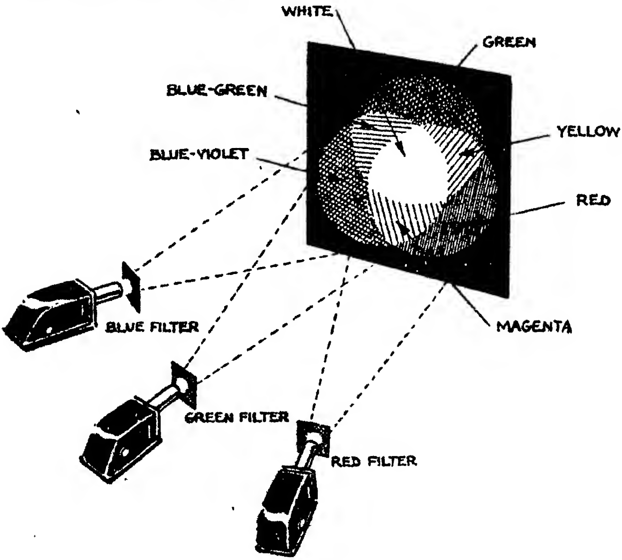


Figure 8

Additive Systems

Soon after Maxwell had propounded the theory that it should be possible to match practically all colours appearing in nature by combining suitable proportions of red, green

and blue light, Louis Ducos du Hauron* described means which made it possible to operate an additive colour process without having to manipulate three separate negatives and three separate positives. Du Hauron realised that if three images could be obtained on a single plate or film, each one occupying one-third of the total area, but at the same time being intermingled one with the other in the form of minute elements, then the eye, being unable to resolve the individual elements at normal viewing distances, would mix the component images in an additive fashion.

The earliest form of additive "screen plate" was made by mechanically ruling very fine red, green, and blue lines adjacent to each other on glass to form a "taking screen" which was placed before a colour-sensitive plate in the camera. A similar "viewing screen" was subsequently fixed to a positive image printed from the negative obtained in the camera, and the resulting colour transparency was either projected or viewed by transmitted light. Later the Lumière brothers sifted starch grains to a uniform size, and dyed them red, green and blue in equal quantities before scattering them on to the "tacky" surface of a plate and covering them with a layer of panchromatic emulsion—exposure taking place through the glass and through the starch grain filter elements. The haphazard mixing of these grains resulted in some "clumping" of similarly coloured elements, with the result that such groups became visible here and there in the picture as relatively large brightly coloured spots.

With most of the "screen-plate" additive processes it is necessary to convert the combined three-colour negative image originally obtained in the camera, into the requisite positive image, and for this a "reversal" form of processing is employed. In this way the film which is exposed in the camera finally becomes the colour photograph itself—one

* Louis Ducos du Hauron had a remarkable influence upon the development of colour photography. Before the end of last century he had either demonstrated or proposed almost all the processes we now use.

reason why it has been easier to produce colour transparencies than colour prints.

After many years of steady improvement, Dufaycolor film, as it is now made, represents the latest, and probably the last, of all the many additive processes. The Dufaycolor filter-mosaic is a remarkable achievement, for the red, green and blue filter elements are so small that more than a million of them are arranged within a square inch, all uniform in shape and evenly distributed over the whole area of the film.

There are several inherent limitations connected with the additive processes, and it seems safe to say that they will all very soon fall into disuse. One difficulty is that an additive transparency cannot be enlarged to any worthwhile degree without its filter pattern becoming obvious. Furthermore, the filter elements absorb a great deal (some 75%) of light, making any additive system inconvenient for motion picture projection and useless for making paper prints, where the total amount of light reflected from the surface of white paper cannot be practicably increased.

Subtractive Systems

It has been possible to make very beautiful colour photographs on paper by "subtractive" methods since the earliest days of photography, although the cost and complication of such processes have restricted their practice to a few professional and still fewer advanced amateur photographers.

Subtractive processes depend upon the removal of appropriate amounts of light of the three primary colours from an original source of white light, which may either be reflected from the surface of white paper or projected though a lantern (Fig. 9). The "appropriate" amounts of red, green and blue light are automatically arrived at by making positive images from the usual set of three separation negatives, and converting each positive into a coloured image which will absorb varying amounts of light of that colour with which the original record was made. In other

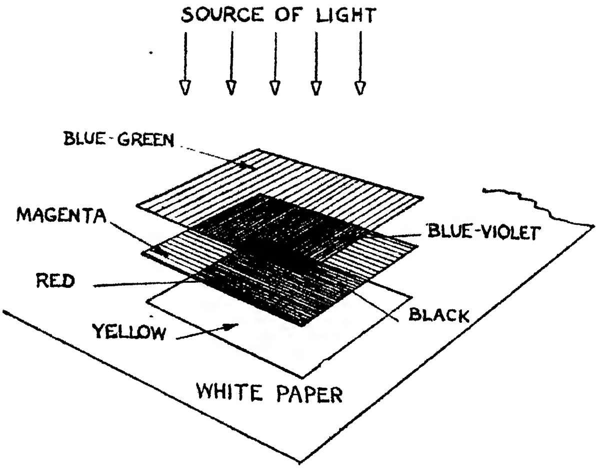


Figure 9

words the positive from the red record is converted into a blue-green or cyan image which will reflect all blue and green rays while absorbing red rays in proportion to the density of the image at any point. The green record positive is converted to a "minus-green" (i.e., magenta) image which absorbs green while reflecting red and blue, and the blue record positive is converted to a "minus-blue" (i.e., yellow) image which reflects both red and green rays while absorbing blue.

For centuries, without concerning themselves with the reasons, artists have been successfully mixing the three subtractive primaries—blue-green, magenta and yellow; although they usually insist upon erroneously calling them blue, red, and yellow.

It will be seen that the need for three records representing the red, the green and the blue components of the subject, is a requirement common to both additive and subtractive systems of colour photography. These "separation negatives" can only be taken one after the other if the subject

is quite still during the time required to make all the exposures, a condition which rules out all but still-life. For active subjects a "one-shot" colour camera is generally employed.

Colour Cameras

A colour camera is essentially an instrument which divides the light from a single lens into two or three paths (Fig. 10). When all three negatives are to be recorded at

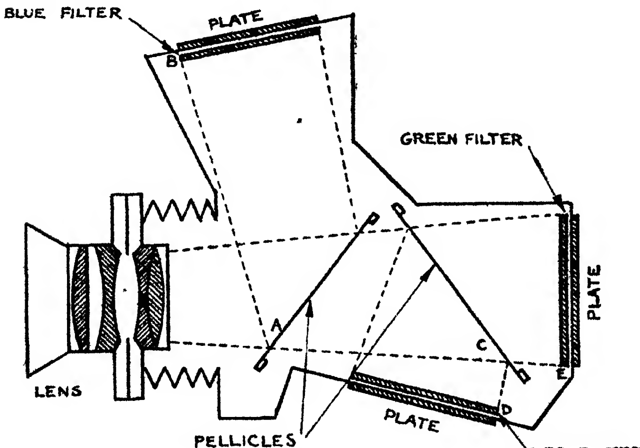


Figure 10

separate exposure planes, two partially transmitting and partially reflecting mirrors must be placed within the camera to intercept the rays emerging from the lens. The ratio of reflected to transmitted light is controlled for each reflector by the degree of "metallisation" which is applied to its surface. Highly reflective metals like aluminium, rhodium and gold are usually deposited on the front surfaces of the reflectors by vacuum evaporation or sputtering.

The reflectors used in "still" colour cameras are known as "pellicles," and are extremely thin (0.0005 in.) collodion

membranes stretched upon optically flat metal frames. Pellicles are superior to glass reflectors in that they do not produce refraction and subsequent distortion of the transmitted image, and do not reflect a second image from their rear surface—the two surfaces being sufficiently close together to act as one.

When it is inconvenient, because of limited space, to introduce two reflectors between the lens and the rear image plane, a dividing system with only one reflecting surface may be used—in which case two of the three required separation negatives are exposed together at one of the two image planes, by placing two films emulsion to emulsion in the form of a "bi-pack" (Fig. 11A). This

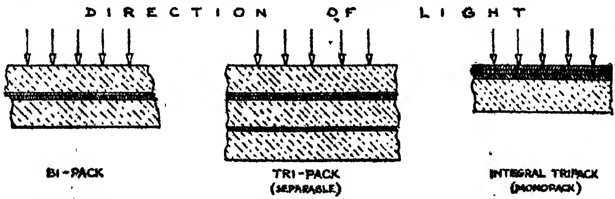


Figure 11a *Figure* 11b *Figure* 11c

arrangement is employed in motion picture colour cameras. The bi-pack usually comprises a front film which is sensitive only to blue light, but which is coated on its emulsion surface with a layer of red-dyed gelatine so that only red rays pass through it to the second film, this rear film being sensitised to red light. The green record negative of the set is obtained on a third film exposed separately (but simultaneously) at the other image plane.

This device, of exposing one film by means of light which has already passed through another, is bound to result in a relatively unsharp image in the rear element, and this has a great deal to do with Technicolor films appearing unsharp on the cinema screen.

Subtractive Printing Processes

After the colour camera has analysed a scene into terms of

three separation negatives, synthesis of it again is effected by means of superimposed coloured images in the complementary colours.* There are several processes by which the appropriately coloured component images can be obtained. Du Hauron, as was usual, made the earliest experiments, and these were based upon the bichromated gelatine or "carbon" process. The "carbon black" gelatine-coated papers used for monochrome printing were replaced by papers coated specially with gelatine containing pigments of the three subtractive primary colours—cyan, magenta and yellow. When sensitised by immersion in a bichromate solution, these tissues were exposed to light behind the appropriate negatives. The action of light on a bichromated gelatine layer does not produce a visible image, but instead, renders the gelatine insoluble in the exposed areas. Thus a positive gelatine "relief" image can be obtained by exposing a sheet of sensitised "tissue" behind a negative, and "developing" it in hot water.

The trichrome carbon process, still in use to-day, is capable of producing high quality colour prints in skilled hands, although the comparatively low speed of the sensitised tissues make it necessary to print the separation negatives by contact—enlarged prints requiring far too long an exposure.

A modification of the carbon process enables ordinary bromide prints (enlarged if necessary) to be used as a substitute for the light action on the sensitised tissue. This procedure, known as the trichrome *carbro* process, is probably at once the most popular and the most difficult of all the indirect methods of making colour prints on paper.

Dye-toning and "metallic" chemical toning processes are often used, the former depending upon the capacity of certain silver salts (silver iodide in particular) to absorb basic dyes in an image-wise fashion, and the latter upon the fact that a metallic silver (black) image can be con-

* The primaries with their complementary colours are: red and cyan, green and magenta, blue-violet and yellow.

verted into a number of other metallic compounds which are themselves coloured. Conversion of the silver image into one of ferric ferrocyanide will provide a cyan component, conversion to a nickel salt followed by treatment with di-methylglyoxime will give a magenta image, while an image formed of cadmium sulphide will contribute the yellow positive.

Technicolor

Another process, important because it is used for the production of Technicolor motion picture prints, is known as the "imbibition" or dye-transfer process. An unhardened gelatine-silver bromide emulsion is coated on film, and is exposed through the celluloid and behind each of the three separation negatives. The latent positive images are next developed in a "tanning" developer such as pyro (pyrogallic acid) which makes the gelatine insoluble in the locality of the reduced silver forming the image. If the developed film is then treated with hot water, the soluble gelatine which remains will be dissolved away to leave a hardened gelatine relief image. At this stage, these gelatine "bas-reliefs," known as matrices, are unlike the gelatine relief images of the carbon or carbro processes since they do not contain pigments or dyes. The three transparent images are immersed in the appropriately coloured dye baths, and after rinsing to remove the adherent surplus, the dyes contained in the matrices can be transferred successively by direct contact on to mordanted gelatine-coated paper or film.

In the production of Technicolor films it would obviously be impossible to obtain each composite colour image by transferring the dyes singly frame by frame, since there are some 14 or 15 thousand individual pictures to an average feature film. Instead, both the dyed matrix film and the gelatine-coated blank film are registered face to face on a flexible metal "pin-belt" which holds the two films in perfect fixed relationship while they travel around the imbibition machine at more than 100 feet per minute.

Tripacks

Colour cameras are inevitably complex, bulky and costly (a motion picture colour camera probably costs some £5,000), and they certainly would not be used if equally satisfactory results could be obtained by simpler means. Most progress towards rendering the colour camera obsolete has been made by the development of various forms of "tripack"—a combination of three films or emulsions on one celluloid base.

Foreseeing the possibilities of a tripack in 1897, du Hauron described it as a device entailing the use of only one plate carrier and one lens, yet producing the three separate negative records by simultaneous selection of the light rays by means of an alternation of colour filters and colour sensitive plates or films, placed together like leaves of a book in the plate-holder.

The principal objection to a tripack of this kind is that the definition of the third emulsion of the set is very poor because it is separated from the other two by the thickness of one film base (Fig. 11B).

However, if the three emulsion layers of a tripack are coated one directly upon the other and all upon a single film base, then since the total thickness of all three emulsion layers need not be much greater than 0.001 inch, definition of the images will suffer very little (Fig. 11C). This "integral" form of tripack is obviously going to be very difficult to manipulate if any attempt is to be made to separate each emulsion layer in turn from the parent "pack," in order to make positive prints. No attempts to work along these lines have yet succeeded on a commercial scale, although two methods of operating in this manner have been disclosed in recent patents.

In 1912, it occurred to Rudolf Fischer that an integral tripack might be used to form a colour photographic image without separating the emulsion layers. Fischer's idea was to make a tripack by coating three layers of emulsion on a common support, the first one being sensitive to red, the

middle one sensitive to green and the outer one sensitive only to blue light. Furthermore, these emulsion layers were to contain substances which, after development, would leave coloured compounds *in situ* with the metallic silver image. These colour images were to be complementary in hue to the colour sensitivity of their respective emulsion layers.

This property of some types of organic developing agents of forming oxidation products which will react with other substances (sometimes in the emulsion and sometimes in the developing solution) to form dyes, was discovered by Homolka, but Fischer was first to propose the practical application of the discovery. The group of developing agents whose oxidation products are capable of forming dyes when combined with suitable "couplers," are known as diamines—diethyl-paraphenylene-diamine being a typical example. In general, the many possible coupling compounds are distinguished by having a reactive methylene group—the cyans are usually phenols, the magentas either nitriles or pyrazalones, and the yellows esters, ketones or amides. Among the couplers enumerated by Fischer in his original patent were: ethyl-acetoacetate (yellow), para-nitrobenzyl cyanide (magenta) and alpha naphthol (cyan), while in recent years many hundreds of new coupling compounds have been found and patented, some of which more nearly fulfil theoretical requirements as regards hue and saturation than did the earlier suggestions.

This idea for the utilisation of an integral tripack was far ahead of the capabilities of photographic materials of the time—for one thing there were no really satisfactory colour-sensitive emulsions available in 1912. But the principal difficulty which confronted Fischer and continued to baffle his successors until the nineteen thirties was that the coupling compounds when introduced into the emulsion layers of the tripack tended to wander from their proper positions into adjacent layers so that correct colour separation was not maintained.

Kodachrome

Mannes and Godowsky, working in America in association with the Eastman Kodak Company, invented a colour development process to avoid the difficulties which arise when couplers are contained in the emulsion layers of an integral tripack. With this process, launched in 1935, and known as Kodachrome, couplers are incorporated in the developing solutions, so that a coloured image is formed at the same time as a silver image is developed. The problem which arises with this procedure is to find means of differentially colouring the individual emulsion layers. At first, this was accomplished by controlling the penetration of the successive colour-developing baths so that each layer was in effect treated separately—a cyan image being formed in all three layers at first, the outer two layers being bleached and then redeveloped to magenta, and the outermost layer again being bleached before final redevelopment to yellow.

The "controlled penetration" procedure was employed in the processing of Kodachrome until 1942, when a considerable simplification was introduced. The present procedure is to develop the exposed film in an ordinary "black and white" developer in order to obtain negative images in all three layers. This means the appearance of black metallic silver in the emulsion, wherever light struck; but the developer leaves the unexposed parts still light-sensitive. After washing, the film is then exposed through its base to red light, which can only affect the previously undeveloped parts of the red sensitive emulsion and thereby render it developable in a cyan colour-forming developer. The film is next exposed to blue rays from the emulsion side, so that only the outer, blue sensitive, emulsion layer is rendered developable in a yellow colour-forming developer. Finally the middle, green sensitive emulsion is fogged and developed in a magenta coupling developer. Both the negative and positive silver deposits are then removed to reveal the composite subtractive coloured image, which is of course a transparency on a film base.

Agfacolor and Anscocolor

Despite the difficulties already described, a "Fischer" type integral tripack with couplers contained in the three emulsions was successfully realised by the Agfa Company in Germany shortly after the introduction of Kodachrome. The Agfa chemists overcame the tendency of the couplers to diffuse from their respective emulsion layers by attaching long chain fatty acid residues to the basic coupler molecules, thereby so greatly increasing their size and moelcular weight (they average from 500 to 600) as literally to "entangle" them within the structure of the gelatine of the emulsion layers. With the couplers rendered immobile, the Agfacolor process (Anscocolor in America) makes use of the simple processing procedure envisaged by Fischer in the first place. After exposure, the film is developed in an ordinary black and white developer, as is Kodachrome, but after washing, unlike Kodachrome, Agfacolor is thoroughly re-exposed simultaneously throughout all three layers, so that all the unused silver halides are rendered developable in the colour developing bath which follows. In the colour developer, containing no coupling compound, the silver halide belonging to all the layers is reduced to silver, and at the same time appropriate subtractive coloured images are formed within the layers *in situ* with the reduced silver. The final removal of the unwanted silver densities leaves the composite coloured image as a transparency.

Kodachrome and Ansco Prints

That both Kodachrome and the Agfa or Anscocolor multilayer transparency materials would be capable of giving subtractive colour prints provided their emulsions were coated on paper instead of on film, seems fairly obvious. In practice, nevertheless, there are additional problems involved. In the first place, the double remove from the original subject, which must result from printing on to a colour-print material from a colour transparency, means

that a considerable loss of colour saturation occurs in the final print. Secondly, paper is an unsuitable base material on which to coat a number of superimposed emulsion layers to a closely controlled thickness, and it cannot be considered as inert chemically as cellulose acetate or nitrate film. Because of this latter difficulty, when printing materials of the Kodachrome and Agfacolor type were introduced, they were in the form of triple layer monopacks based on white pigmented cellulose acetate film—rather like a playing card in feel and thickness.

The Eastman Kodak Company now make Kodachrome prints at various degrees of enlargement from Kodachrome transparencies, while Ansco (also in America) sell printing materials with which the user can make prints from either Agfacolor, Anscocolor or Kodachrome transparencies. The reason that Ansco Printon, as the latter product is called, can be processed by the amateur or professional photographer is that the necessary colour couplers are contained in the three emulsion layers of the colour "paper" and only require printing and fairly simple reversal processing, first in an ordinary developer and then in a single colour developing bath, to produce the finished print.

Several large processing laboratories have been set up in America to produce colour prints from amateurs' colour transparencies, and these organisations all appear to depend upon the Ansco Printon material. The sensitised material is printed and continuously processed in roll form, and prints, at fixed degrees of enlargement, are produced at the rate of one hundred a minute.

Negative-Positive Kodacolor

In 1942, Kodacolor film made its appearance, and was the first subtractive colour material ever to be sold for processing to a "colour-negative" from which any number of colour prints can be made. Kodacolor film is again a realisation of Fischer's ideas. The coupling materials, instead of being "weighted" with large molecules as in

Agfacolor, are suspended in minute particles of resinous organic materials—insoluble in water and therefore non-diffusing, but sufficiently permeable to the developing solutions to permit the necessary reactions of the colour development process to proceed.

After exposure Kodacolor film is returned via a dealer to the laboratories of Eastman Kodak, where it is developed directly in a colour developer to a colour-negative image. This idea of a colour-negative is not easy to grasp, as not only the light and shade of the picture is negative, but in addition, the colours of the negative image are complementary to those of the original subject and to those which will appear in the final print. For example, in a Kodacolor negative a blue sky will appear yellow, a red dress is blue-green, yellow flowers will be blue, while normal flesh colours will appear a greenish hue. The complementary coloured negative is printed on to paper carrying three emulsion coatings of very much the same kind as those on the original film. The "minus" colours of the negative are automatically separated by the differentially colour sensitive layers of the printing material, and upon colour development of the exposed paper and removal of the unwanted silver images, a print results which is positive in tone values and correct in colour.

The printing and processing of Kodacolor snapshots (for the amateur is the person for whom Kodacolor is intended) is carried out on continuously operated machines at speeds which will result in the hundred million colour prints a year already mentioned in the opening paragraph of this survey.

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The Compass

COMMANDER W. E. MAY, R.N.

THE compass is an instrument for indicating direction at sea, on land and in the air.

Of all the inventions which have been of benefit to mankind no other can have had so profound an effect upon history, yet few are so taken for granted. To the great majority of people a compass is just a compass and there are not many who realise the inventive work that has been applied to the design of these instruments, the mechanical skill of those who make them or the immense amount of thought and time needed to ensure that they are properly fitted and function with accuracy.

Compasses fall into two main headings; the magnetic compass, which owes its directive properties to the magnetism of the earth, and the gyro-compass, which is operated by forces set up by the rotation of the earth and the force of gravity.

The Magnetic Compass

The magnetic compass, in its simplest form, consists of a bowl in which a magnet or system of magnets is supported on a central pivot. This magnet system carries a card which is marked with the points of the compass. These are read against a pointer, known as the lubber's point, showing at any moment the actual direction of the ship's head. In modern compasses the bowl is now always filled with liquid, which has two purposes: firstly, to reduce the weight of the magnet system, as will be seen hereafter, and, secondly, to act as a brake to damp the oscillations of the card. The magnet system is formed by one or more needles attached to a card and a float. The copper float is of such size that it allows the magnet system just to sink in the

liquid used. By this means the system has an almost negligible weight on the pivot, and friction and wear are consequently reduced. In early compasses only one needle was fitted, but it was found that if two needles were fitted, spaced about half their length apart, the system was balanced and became more stable. More needles can be used, but two or four is now the usual number. Their proper distance apart is governed by certain mathematical rules which are too complicated to go into here.

The great advantage of fitting more needles is, of course, that the strength (known as the magnetic moment) of the system can be increased in this way without making them inconveniently long.

In designing a compass, it is desirable that the magnetic moment should be fairly high to overcome the friction of the pivot. On the other hand, there are disadvantages in making it too great. Another point in the design is that the time taken by an oscillation of the needles, if they have started swinging, must be fairly long (about 20 seconds), as should this time of swing coincide with that of a roll of the ship the compass would be unsteady.

In the centre of the float is a cap which contains a jewel, and this rests on a pivot rising from the bottom of the bowl. These provide an almost frictionless form of support.

The card is fitted exactly at the height of the pivot point so that it does not move sideways should it tilt relative to the bowl. In order to make the magnet system as steady as possible in a seaway it would be preferable also to fit the needles with their centre of gravity at the height of the pivot. Unfortunately this is not practicable. The magnetism of the earth only acts horizontally at the magnetic equator. In other latitudes it acts at an angle to the horizontal and would consequently cause a balanced magnet system to tilt with its north pole *down* in northern, and *up* in southern, latitudes. The remedy is to sling the needles below the level of the pivot. The magnet system is thus

made bottom heavy and this bottom heaviness prevents it tilting in any latitude.

In a well-designed liquid compass, the card is nowadays always made of considerably smaller diameter than the bowl. When the ship yaws, the liquid in the outer portion of the bowl is carried round with it, while that in the centre remains more or less stationary. If the card is small enough it will remain in this area of comparatively still liquid, whereas a larger one would reach the area of disturbed liquid and so be disturbed itself.

There is little doubt that liquid compasses would have been introduced very much earlier were it not for difficulties

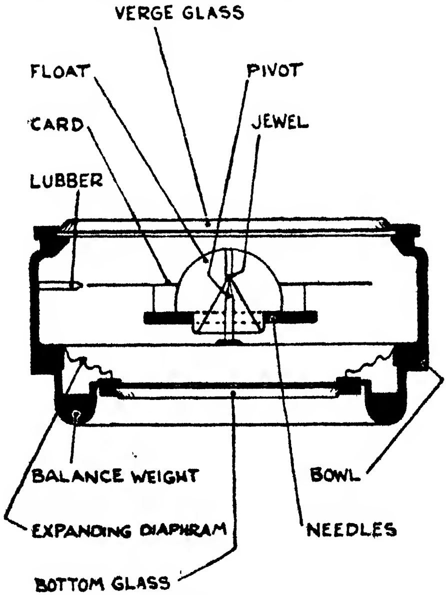

Figure 12

of manufacture. The chief difficulty is that the volume of the liquid varies with the temperature, and unless provision

is made the compass develops large bubbles in cold weather or bursts during hot. For this reason it is necessary to fit some form of expansion chamber, and in British naval compasses this is achieved by attaching the bottom of the bowl to the sides by a corrugated ring, which allows the bottom to move up and down as the liquid expands or contracts (see Fig. 12).

The liquid is usually a mixture of water and methylated spirit. The addition of the methylated spirit prevents the water freezing at low temperatures.

At one time a legend grew up in the Royal Navy that if a bubble occurred in your compass you should fill it with neat gin. By some extraordinary means this came to the ears of the masters of Arab dhows trading to the Persian Gulf. It became a custom to appear on board one of H.M. ships with an empty compass and to ask that it might be refilled. After a while there came an officer who not only knew the correct mixture but also realised that in the Persian Gulf a compass could not possibly freeze whatever it was filled with, and so started filling their empty compasses with distilled water. It is understood that dhows' compasses no longer leak.

The bowl is fitted with a glass top so that the card may be seen and a glass bottom so that it may be illuminated by a light from below. It is slung by means of a ring in two pairs of pivots at right angles to each other. This arrangement, which is known as gimballing, keeps the bowl horizontal, however much the ship rolls.

In old days compass cards were graduated into "points" and "quarter points." There were thirty-two points, so that one point equalled 11¼ degrees. With the advent of steamships and more accurate steering, even the quarter points became too large for convenient use, and the system of marking cards and steering by degrees gradually came into use. There was a period during which cards were marked both in points and degrees, but the former are now gradually being omitted. A modern card is marked in degrees only,

with letterings for seven of the eight principal points, and a fleur-de-lys to indicate the North. When degrees were first introduced they were numbered in 90-degree quadrants from North and South in both directions to East and West, as this system fitted in with the manner of naming points. At a later date graduation all the way round in one direction from 0 to 359° was introduced in some compasses. The Royal Navy formerly preferred to stick to the old method for magnetic compasses, reserving the new for the gyro-compass, thus avoiding any confusion, but will shortly fall into line with the many nations which use the 360° notation.

Variation and Deviation

The magnetic compass does not point to the North Pole but roughly in the direction of the North Magnetic Pole, following the lines of the earth's magnetic field. The angle by which the compass needle is deflected from true North is known as the *variation* and this error must be allowed for when using a magnetic compass. Charts are published showing the value of the variation all over the world and also the annual amount by which it changes, for the variation does not even remain constant in any one place. At places between the true and magnetic poles of the earth the variation is 180° and the compass will actually be pointing true South.

If any iron or magnetic material is brought near the compass the needle will be deflected. Since ships are now built of steel their compasses tend to be very much in error. The angle by which the needle is deflected from the correct magnetic direction is known as *deviation*. The value of this deviation will depend on the position of the compass in the ship, on her course and on her position on the earth. To reduce the deviation to one or two degrees it is customary to correct the compass by means of horizontal magnets placed fore-and-aft and athwartships, by vertical magnets below the compass and by spheres and bars of soft iron. In fact if such correction were not made many compasses

would point permanently to one part of the ship and so be useless. Unfortunately the magnetic quality of a ship is always changing and consequently it is necessary to readjust her compasses at least once a year. To reduce the effect of these changes it is necessary to give considerable attention to keeping any iron-work, electrical instruments, etc., as far from the compass as possible.

War-time Difficulties

During the late war a further difficulty occurred. To combat the magnetic mine, coils were wound round every ship of any size and these, when energised by electric current, neutralized the magnetic effect below the hull so that mines passed over did not explode. Unfortunately, from the navigator's point of view, these coils also of course affected the compass, causing large deviations. To overcome this new difficulty small coils were fitted beside the compass and were fed with current from the same source as that supplying the protective coils. These compass coils of course acted as electro-magnets the strength of which could be adjusted by having a variable resistance in series with each. The fitting of these coils needed an immense effort on the part of those who were responsible for compass matters and, besides doubling the time needed for each compass adjustment, meant that compasses had to be corrected far more frequently.

The Gyro-compass

During the nineteenth century great advances were made in ship design and ship construction. These brought a series of compass problems for the navigator. The most obvious of the new difficulties were the errors caused by the magnetism in the steel hulls and the effect of the vibration of the engines, but these were not all. The new warships which were being built were no longer content with a single steering position. No longer did the captain of a battleship walk his open poop throughout an action. He now had

an after bridge from which to handle the ship, if the fore bridge were shot away, and heavily armoured conning towers at each to retire into when firing became heavy. Compasses were needed at all these positions and a battleship might need as many as ten.

At each of these compass positions there were the conflicting interests of a large non-magnetic space to ensure the compass working, and the necessity of armour plating close around to protect the man who was going to use it. In addition, there was the time required to adjust all these numerous compasses and to provide a table of errors for each. It will readily be understood that inventors had every inducement to provide either a compass working on some new principle and unaffected by magnetism, or a means by which repeaters could be placed about the ship to show the heading of a single magnetic compass which could be ideally positioned.

The second problem appeared to be the easier and many ideas were patented. Of these we may mention that of Jenkin in 1863, who required 100 wires to join each repeater to the master compass. A satisfactory arrangement was not, however, introduced until recently. The invention of a satisfactory non-magnetic compass had been achieved some years earlier. It is, of course, the gyro-compass to which we refer.

In 1851 the French scientist Foucault demonstrated that a heavy pendulum freely suspended would, if once set swinging, continue to swing in a direction fixed relative to space and not to the earth. For instance, if such a pendulum were suspended at the North Pole and started swinging it would appear to follow the sun around and not always swing towards England or whatever direction in which it had been started.

In the next year Foucault invented his gyroscope, a logical development of his pendulum. A gyroscope is a heavy rotating wheel so suspended that it is free to turn in any direction. It possesses two peculiar properties. Like the pendulum, it will always endeavour to keep its axis pointing

in the same direction in space. If it were started with its axis pointing towards a particular star it would keep pointing at that star and in the course of twenty-four hours would appear to the observer to describe a cone, the axis rising as the star rose and sinking with it; continuing to point down through the earth at it as the rotation of the globe made the star seem to pass behind the earth, ready to rise in the east again the next night.

The second property of the gyroscope is that if pressure is applied to its axle the axle will not give way in the direction of the pressure, but will move in a direction at right angles, i.e., if an attempt is made to push the axis round horizontally it will tilt instead, and *vice versa.*

It was not long before men saw that the properties of the gyroscope might be used to provide a direction indicator for use at sea, and the first instrument of this nature was patented by Smythies in 1856. Sufficient progress had not yet been made in other directions; the problems of driving the gyroscope and of providing sufficiently free bearings were too great, and it was not until 1905 that Anschütz, a German, introduced the beginnings of a satisfactory gyro-compass.

He was followed a few years later by Sperry, an American, whose compass, after years of disappointment, was made, with British help, into a satisfactory instrument. Other gyro-compasses are the British Brown, the American Arma, the French Monfraix and the Italian Florentia.

The Compass Described

The modern gyro-compass can now briefly be described. The kernel of the instrument is a heavy rotor driven at high speed by alternating electric current. The Sperry rotor weighs about 50 lb. and rotates 8,600 times per minute. The other makes have smaller rotors weighing about 4 lb. and rotating about twice as fast. This rotor is freely suspended, and if started with its axis pointing in any other direction than North horizontally, would describe the cone

mentioned earlier. However, it is arranged so that as soon as the axis tilts relative to the earth, the force of gravity causes a weight to exert pressure on one end of the axis, making it move at right angles and precess, as it is called, towards North. In the early Sperry an actual lead weight was used, but it was found that this introduced errors by trying to swing about when the ship rolled. All modern compasses use liquids which, flowing from one receptacle to another, produce the necessary movement of weight.

The rotor with the containers for its controlling fluids form what is known as the sensitive element and this must be very freely suspended. It is usually suspended in a framework called a phantom, which is driven round by an electric motor so that it always keeps in step with the sensitive element. The methods by which the element controls the phantom to make it follow vary considerably. Only the lightest form of connection can be tolerated. Rubbing contacts, switches controlled by air or magnets and A.C. transformers have all been used.

The motor which drives the phantom is also used to drive a transmitter which sends out impulses to repeaters which can be placed anywhere in the ship and which show the ship's head as indicated by the master compass. It is usual to send out impulses to the repeaters six times to a degree, but other speeds of drive are also used (see Plates 12 and 13).

Let us now consider what happens when a ship alters course. As she turns she carries the phantom round with her while the gyro remains pointing to the North. Thus the phantom and sensitive element are no longer in line with each other and this lack of alignment causes a signal to be sent to the motor so that it drives the phantom back in line again. At the same time, the transmitter is driven round and sends out six impulses for every one of the degrees through which the phantom moves to keep in step with the rotor. Thus all the repeaters get the requisite number of impulses to make their cards move in step with the master also.

The above is the main outline of working, but there are

many complications. Special arrangements must be made so that the gravity control brings the rotor axis quickly to north instead of oscillating about that direction. The transmitter will drive only a limited number of repeaters, and if more are required relays must be introduced. Where an A.C. transformer is used to control the follow-up system a complicated arrangement of valves is needed to amplify the signal.

Not Used in Aircraft

The gyro-compass has an error which depends on the speed and course of the ship. It is very susceptible to shocks and violent accelerations. The greatest possible degree of freedom is necessary for all its bearings, and as the smaller the instrument the better these must be, manufacturing considerations preclude reduction in size of the instrument to any great extent. All these reasons have so far prevented the use of the gyro-compass in aircraft, although for ship use the modern gyro-compass is an extremely reliable and accurate instrument. From time to time wonderful accounts have appeared in the press of gyro-compasses used in aircraft by the Americans and ourselves. The instruments to which they referred were not gyro-compasses in the generally accepted sense but magnetic compasses controlling repeaters, and having a gyroscope included in the system for some purpose other than making the compass north-seeking. In one well-known type the gyroscope stabilises the magnetic element to keep it level while in another it is interposed as a link in the transmission and serves to smooth out irregularities. Such compasses are primarily magnetic compasses with many of the disadvantages of that type.

At the present day the use of the gyro-compass at sea is rapidly increasing and soon few well-found ships will be at sea without them. As, however, they depend on an external supply of electricity, which may fail, and they take about five hours to find the North again if disturbed, it is improbable that any owner would be so foolhardy as to send a ship to sea without a well-placed and properly corrected

magnetic compass to fall back on in case of need. This remark of course applies to merchant vessels. In ships of war the difficulty of finding a suitable position for a magnetic compass sometimes leads to its omission. In such cases more than one gyro-compass is fitted and the chance of all failing together is reduced by the number of skilled ratings on board.

Influenza

WILSON SMITH

FOR those old enough to remember the first world war the name influenza has a significance which younger generations find difficult to appreciate. The pandemic of 1918–19 was possibly the greatest single calamity which has ever befallen mankind; in a few months it took more toll of human life than did the war itself in over four years. Ignorance of the primary cause of the disease, coupled with the lack of any rational preventive measures, engendered a feeling of complete helplessness; no wonder therefore that both during the epidemic itself and in subsequent years strenuous efforts were made to solve the many problems which influenza poses to the research worker.

Of course, even before 1918 a great deal was known about influenza. We have fairly reliable records of major outbreaks occurring at intervals since the 16th century with some excellent descriptions of the various forms the disease may assume, the complications which may follow an attack and the modes of spread through communities and from place to place. But knowledge of the essential cause was lacking, without which investigators were largely groping in the dark. It is not always necessary to know the cause of a disease in order to control it; in smallpox vaccination we have an example of a potent weapon of control utilised long before the virus of smallpox had been discovered. Nevertheless isolation of the causative organism is, in most cases, the first essential step towards the development of rational control measures.

Types of Influenza

Influenza is not a single disease, like diphtheria or whooping cough, with a single causative organism. The name is used

to describe any infection of the respiratory tract which produces a certain characteristic group of symptoms. But, although the clinical picture may vary from case to case and in different outbreaks, some symptoms may be regarded as typical of influenza as a whole. These are a sudden onset of headache, shivering, muscular pains and giddiness, with loss of appetite and severe prostration. The patient's temperature soon rises and he develops a dry cough from inflammation of the throat. In uncomplicated cases the illness passes off in a few days leaving the victim feeling somewhat weak and depressed, but otherwise none the worse.

This type of infection occurs in three main forms. In endemic influenza scattered cases occur every year, mostly in the winter months, in practically all centres of population. The disease is not highly contagious, for far more people escape than are affected, and if influenza confined itself to this form it would not be regarded very seriously. The second type is epidemic influenza. Every two or three years in the late autumn, winter or early spring, outbreaks occur wherever people are crowded together in close contact. The disease seems to explode suddenly, the numbers of fresh cases each week increasing rapidly to a peak and then almost as quickly declining, the whole event being over in about 6 or 8 weeks. Although not a very serious illness in itself, this epidemic influenza may be responsible for many deaths amongst those already suffering from other ailments, especially the aged and infirm; moreover its interference with normal activities constitutes a source of great economic disturbance and loss. Lastly the pandemic form appears as a bolt from the blue at quite irregular intervals; starting from a primary focus in any country it sweeps across all boundaries through the entire world. Persons of all ages and conditions are attacked, in many of whom the disease takes on a much more serious character, often involving the lungs with the development of an influenzal pneumonia. Even in such major pandemics

many more victims recover than die, but the visitation is on such a vast scale that the mortality rate assumes alarming proportions. Two or even three waves of infection may follow each other in close succession, after which the pandemic seems to burn itself out, leaving mankind resistant to further attacks of like nature for many years.

Influenza Problems

This very brief résumé of the main types of influenza may help towards an understanding of the major problems which confront research workers and which may be summarised as follows:—

(1) Etiology. What is the primary cause of influenza? Are the three types caused by similar organisms or by quite different ones?

(2) What happens to the organisms responsible for epidemic and pandemic influenza between outbreaks? From whence do they come at the beginning of an outbreak?

(3) Why do epidemics attack a community every two or three years, whilst pandemics arise only at long irregular intervals?

(4) What determines the fatal nature of many of the cases during a pandemic?

(5) How can individuals be protected when exposed to infection, and how can epidemics be either prevented altogether, or brought rapidly under control when they occur?

In addition to these there are of course other major problems and innumerable subsidiary ones. Some have been partially solved during recent years, others have so far defied all efforts at solution and others again must await the occurrence of the next great pandemic before they can even be investigated. Research during the last decade has been concerned mainly with epidemic influenza, for the very good reason that the etiological agent of this form only has yet been discovered; the causative agents of the endemic

and pandemic forms still remain unknown. Unless this fact is clearly borne in mind, appraisement of the true significance of results so far obtained is impossible.

Discovery of an Influenza Virus

Before the pandemic of 1918–19, many bacteriologists believed that influenza was caused by infection with an ordinary microbe named *Haemophilus influenzae*, or more commonly, the influenza bacillus. Researches during that pandemic, however, showed fairly conclusively that, although this bacillus may have played some part in many serious cases of the disease it was certainly not the primary or essential cause. Already at that time there were some who suspected that the causal organism might belong to a different class of infectious agents known as the viruses. These are very much smaller than bacteria, so that they will pass through filters which hold back such microbes. They are too small to be seen by ordinary microscopes, though they can now be photographed by means of the electron microscope described in the first number of *Science News*. Also they will multiply only inside living animal or plant cells and are extremely particular in their choice of the species of animal or plant which they will infect and the type of cell they will grow in. This makes their isolation and investigation difficult and largely accounts for the slow progress of influenza research.

The first isolation of a human influenza virus was accomplished by a team of British workers during the epidemic of 1932–3. This was achieved by dropping washings obtained from the back of the nose and throat of typical cases of influenza into the nostrils of ferrets. The inoculated ferrets developed an illness very similar to the human disease, with high temperature, lassitude, muscular weakness, loss of appetite, and nasal discharge and obstruction. Healthy ferrets brought into close contact with sick ones promptly caught the infection; because of this it was necessary to adopt the most rigorous system of isolation

for all animals used during this investigation (see plates 10 and 11).

The most convenient way of passing on the infection serially was found to be by inoculating healthy ferrets intranasally with material taken from the nasal passages of sick animals and the fact that such material was still infective after all bacteria had been removed by filtration afforded good evidence that the disease agent was a virus.

The mere production of a transmissible influenza-like disease in ferrets did not, of course, constitute proof that the virus concerned was the virus of human influenza. In experiments of this sort there is always a danger that some virus already in the body of the experimental animal will be awakened into activity and subsequently passed on from animal to animal. This indeed has occurred several times in the course of research on various virus diseases and has led to claims which later on have been proved unfounded. But the original investigators supported their claim to have isolated a human influenza virus by further important evidence. The ferret disease was produced with the throat washings of no less than five out of eight patients investigated but could not be produced with washings obtained either from normal persons or from convalescent patients. After recovery a ferret could not be infected again for some time, but this resistance slowly declined, so that several months later the animal became once again susceptible. Here was one clue to the solution of the problem of why some people catch influenza every time an epidemic comes along whereas in the case of measles a first attack is usually the last. Apparently influenza virus is incapable of inducing the solid long-lasting immunity against subsequent attack which characterizes many other virus and bacterial infections.

The immune state following an attack of an infectious disease is usually associated with, and largely dependent upon, the production in the blood serum of substances called antibodies. These have the property of combining

with the agent which incites their production and, in the case of an infectious agent like a virus, the combination results in its neutralisation or inactivation, thus assisting the body to overcome any further attempts at invasion. Such antibodies, capable of neutralising the isolated influenza virus, were regularly found in all ferrets which had recovered from the experimental disease, but the really significant finding was that they could likewise be demonstrated in the blood sera of convalescent human cases, and sometimes even in the sera of normal persons who had passed unscathed through the epidemic. These findings began to shed a little light on the question: Why do some people escape during an epidemic though fully exposed to infection?

After this first report of the isolation of an influenza virus in 1933, it was not long before other virus strains were obtained from epidemics in many different countries and all the claims made by the English workers were fully confirmed. Research workers all over the world could now readily obtain the virus with which to carry out all manner of investigations on the various problems outlined above.

Influenza in Mice and Eggs

Although the ferret proved invaluable for primary isolations of virus from human cases, and still remains essential for some investigations, it is not a very suitable animal for certain types of experiment. It is costly and requires elaborate measures of isolation to ensure that influenza infection is not picked up accidentally. Strenuous efforts were therefore made to find other means of propagating and studying influenza virus. If throat washings from a human case of the disease are inoculated directly into other species of experimental animal, they usually produce no effect, but after passage through ferrets the virus often acquires wider powers of infection. Such ferret passage virus, as it is called, can be used to infect mice. The result is an influenzal mouse pneumonia which, fortunately for

experimental work, is never spontaneously transmitted from mouse to mouse by mere contact. Having once acquired mouse virulence, the virus can be passed on from mouse to mouse simply by grinding up the infected lungs in a little broth and dropping a little of the lung suspension, or a filtrate made from the suspension, into the nostrils of anæsthetised animals. The influenzal pneumonia which results is often fatal, but very small doses of virus produce patches of lung consolidation from which the mice recover, after which they are resistant to further infection with the same virus strain. Mice are thus very convenient animals for measuring the strength of virus suspensions and the amount of neutralising antibodies contained in samples of serum.

Another way in which influenza virus can be propagated when once isolated from human cases is by growth in chick embryos. If virus is inoculated into certain sites in the fertile hen egg at the right stage of embryonic development it will multiply and cause the formation of visible lesions in the embryo or its appendages. For example, inoculation of a suitable dose of virus filtrate through the shell onto the respiratory membrane of the embryo results in the development of small pocks scattered over the surface, which can be counted to give a fairly accurate measure of the number of virus particles inoculated. The lesions can be reduced or prevented altogether if immune serum is mixed with the virus before inoculation, so here again we have a simple means of measuring the antibodies contained in the blood of both normal and convalescent human beings (see plates 8 and 9).

An indication that all strains of epidemic influenza are not exactly alike is that a few of them have been isolated directly from human cases by either mouse or egg inoculation without preliminary passage through the ferret. One of the greatest difficulties in influenza research arises from this lack of uniformity in virus behaviour and from the constant tendency of the viruses to change in some way when transferred to a new environment. A virus may be

very infective for ferrets when first isolated but after several passages through eggs it may, whilst acquiring increased virulence for the chick embryo, become much less virulent for the ferret and completely non-infective for human beings. This sort of change is called mutation, and the altered virus a mutant strain, and in view of the readiness with which influenza viruses mutate, it is not surprising that different epidemics show such diverse characters.

Reactions in Test Tubes

The work discussed so far involved infection of experimental animals, and there is no doubt that without such animal experimentation the attack on our major influenza problems could never have begun. But a few reactions have been discovered which can be carried out in test tubes and are so simple and rapid that they have helped enormously to speed up influenza research, especially in problems involving the detection and measurement of serum antibodies. The most important of these is the red cell agglutination reaction. If the red blood corpuscles of certain species, particularly fowls and group 0 human beings, are washed and suspended in salt solution they slowly fall to the bottom of the containing tube to form a compact mass which disperses again evenly and rapidly when the tube is shaken. If influenza virus is mixed with the red cell suspension the corpuscles tend to stick together in small clumps which settle out more rapidly and are not so easily dispersed. Within limits the greater the amount of virus present the more rapid and complete is this clumping or agglutination, so that the reaction can be used to measure the strength of a virus preparation. The incorporation in the mixture of any serum containing antibodies to the virus prevents agglutination from taking place; apparently the antibodies unite with the virus so that it is unable to attach itself to the red cells. Thus we can quickly ascertain whether John Jones possesses influenza antibodies in his blood serum and, if so, whether he possesses more or less

than Mrs. Jones. Also if we take the trouble to get samples of his blood both at the beginning of an illness and again after his recovery, we can discover whether he has been busy manufacturing antibodies in the meantime. Any marked increase provides pretty strong evidence that influenza virus was the root cause of his illness.

Another use of the reaction is to study the distribution of antibodies in a population before, during and after an epidemic. Most normal adults possess antibodies in some degree, but some show a much higher level than others, possibly because they have had an attack of 'flu more recently. By and large, those with a high level are less likely to succumb during an epidemic than those with a low level, or those without any detectable antibodies at all, but this correlation is by no means absolute. One can never give a guarantee that an individual is certain to escape infection, because the particular kind of antibody in his blood may be of far greater importance to him than the total amount present, as will be appreciated from what follows.

Different Kinds of Influenza Virus

By means of the various animal and test tube reactions described it is possible to analyse to some extent the constitution of different strains of virus and thus find out whether they are all so closely related that they should be regarded as representatives of a single species or are so dissimilar that we must recognise more than one influenza virus. This is of fundamental importance because, if there are several quite unrelated agents of the disease, we cannot expect that a person who becomes resistant to one of them, either by passing through a natural infection or from artificial vaccination, will enjoy any special protection during an epidemic caused by another virus altogether.

The analysis of strains can be carried out in several ways, but all depend upon either estimating the resistance of recovered animals to reinfection or estimating the virus-

neutralising power of their sera. The first type of test is called an active immunity test and is illustrated by the following experiment. Thirty mice were infected with a dose of virus strain X not large enough to kill them. After they had completely recovered, 10 were reinoculated with strain X, 10 with strain Y and 10 with strain Z, care being taken that in each case the dose was sufficient to kill normal control mice. After a week, surviving animals were killed and their lungs examined for signs of influenzal pneumonia. The results may be conveniently set out in tabular form.

Mice recovered from infection with strain X	Died	Lung lesions in survivors *	†	‡	Total No.
Reinoculated with strain X	0	0	0	10	10
,, ,, ,, Y	1	1	3	5	10
,, ,, ,, Z	10	0	0	0	10

*=Extensive lung lesions. †=Slight lung lesions.
‡No sign of any infection.

The original non-fatal infection with strain X had produced a complete resistance against reinfection with the same strain, a partial immunity against strain Y and no protection whatever against strain Z. Obviously the three strains are not identical but, whereas X and Z are completely different, X and Y are to some extent related.

The second type of test, called the serum neutralisation test, is similar in principle. Immune sera to all the strains are obtained from either recovered or vaccinated animals, and then tested to compare their powers of neutralisation against each of the different strains. If we indicate strong neutralising power by N, weak power by W and lack of any neutralisation by O we would obtain, with the same strains used in our active immunity experiment, this result.

		Immune sera X	Y	Z
Virus strains	X	N	W	O
	Y	W	N	O
	Z	O	O	N

Until 1940 such analysis indicated that all the virus strains then isolated, whilst not identical, were sufficiently related to be classed together as one virus type called influenza virus A, but in that year the isolation of a completely different type, influenza virus B, was reported. These relationships can be represented somewhat crudely by a scheme such as the following in which capital letters indicate different components of the viruses and small letters the antibodies corresponding to these various components.

	Virus constitution				Corresponding immune sera			
A strains	P	Q	R	S	p	q	r	s
	P	Q	X	Y	p	q	x	y
	F	G	R	Y	f	g	r	y
B virus ..	L	M	N	O	l	m	n	o

The B virus was derived from epidemic influenza cases which were quite indistinguishable clinically from cases due to A virus infection. Subsequently it was found that not only may different outbreaks be caused by these different types, but that both A and B viruses may be at work side by side in one and the same epidemic. This, of course, very greatly complicates the problem of finding some practical means of protection against epidemic influenza.

Some time before a virus was obtained from cases of human influenza, one was isolated from pigs suffering from the disease now known as swine influenza, or hog 'flu. This swine infection was quite unknown prior to 1918 when it suddenly appeared in devastating epizootic form amongst herds in the middle west of the U.S.A. The great human pandemic was raging at the time and the name hog 'flu derived from the similarity of the human and swine diseases. So much alike were they that shrewd observers suggested that the new disease might have arisen by a transfer of human influenza to swine. Whatever may be the truth about its origin, there is little doubt that swine influenza is related to the human disease in some way because its virus is quite definitely related to the human

type A strains. It produces typical ferret influenza, can be adapted to mice and chick embryos and can be shown by cross immunity and cross serum neutralisation tests to possess components in common with human virus. But the most interesting thing about this swine virus is that it is not the sole cause of hog 'flu. It is able to infect pigs but gives rise merely to a mild infection, perhaps more analogous to the common cold than to pandemic influenza. For production of the serious epizootic disease participation by another agent together with the virus is essential, and this second agent is an ordinary bacterium which is practically indistinguishable from the influenzal bacillus which was once considered the causative organism of human 'flu. Does this solve the problem of the etiology of human pandemic influenza? Definitely not; but it certainly focuses attention on intriguing possibilities. It may be that great pandemics arise only when some chance circumstance favours the formation of an alliance between one of the influenza viruses and a bacterium. Or it may be that bacteria never assume a primary rôle but that virus mutation occasionally results in an agent of extraordinary virulence. Perhaps the final answer will be found in the next world-wide outbreak.

Another fundamental problem has been elucidated in the case of swine influenza which still remains a mystery in the human disease, namely the means whereby the virus is carried over from one epidemic to the next. The swine virus is able to invade a parasitic lung-worm of pigs, and still survives when the lung-worm larvæ are excreted and ingested by earthworms. Months later, fresh litters of normal healthy pigs grub up and eat the earthworms, and the lung-worms, containing virus, make their way from the intestines to the lungs. Nothing happens until the virus is provoked into activity by some stimulus which is not yet known; then if the influenza bacillus is ready to hand the pig goes down with an attack of the disease and can hand it on to others by the usual method of contact infection.

Can some such mechanism be involved in human influenza? It seems extremely unlikely, for we know of no common human parasite with a life cycle adapted for such a rôle. It would seem more probable that some persons carry virus in their respiratory passages without suffering any ill effects, just as apparently normal individuals may carry the germs of diphtheria or scarlet fever. But it must be admitted that carriers of influenza have never been detected during inter-epidemic periods, and until this is done it is advisable to keep an open mind on the question.

Protection against Influenza

In the case of some infectious diseases we can confer a high degree of protection on individuals by vaccinating them with either killed bacteria or certain products of bacteria. All soldiers, before being sent abroad, receive protective inoculations against typhoid fever, and school children are now immunised against diphtheria on an extensive scale. Early in influenza virus research it was found that the vaccination of ferrets or mice with killed virus was followed by resistance to experimental infection, associated with the development of antibodies in their blood serum. Even a vaccine composed of living virus could be safely used because of the non-infectivity of influenza virus unless introduced directly into the respiratory tract. But for efficient protection the virus strain used to make the vaccine had to be the same as, or very closely related to, the strain used to test resistance, so that vaccination with A vaccine conferred no resistance against infection with B virus, and *vice versa*. These animal experiments provided a great deal of valuable information, but the crucial tests of any new remedy must be carried out on human beings. Already many human vaccination trials have been made, and they provide a long tale of frustration and disappointment. Quite apart from the risks which would be involved in deliberately infecting numbers of volunteers, the real value of a vaccine can only be judged by comparing the incidence

of influenza in previously-vaccinated and in control groups during a naturally occurring epidemic. Sometimes, after the organisation of a trial and the vaccination of large batches of volunteers, the expected epidemic failed to materialise, or the outbreak failed to spread to the community under investigation. Sometimes the epidemic exploded before the vaccinations had been completed. In some trials the outbreak was so mild that too few cases occurred to allow any judgment. Then again, the virus strain of the epidemic would be found to be so little related to the virus used for the vaccine that adequate protection was not to be expected. However, enough results have now been obtained to show that vaccination does confer increased resistance to epidemic infection, provided that the epidemic virus is closely related to the vaccine strain and that the interval between vaccination and exposure to infection is not too long. These provisos indicate how very far we are yet from the practical control of influenza along this line of approach.

A group of Russian workers obtained very good results by giving a large number of individuals repeated inhalations of vapourised immune serum, rich in virus antibodies, both before and during an epidemic, but here again the varying constitution of virus strains raises difficulties in any attempt to apply the method on a large scale. Unless the serum employed were to correspond closely to the epidemic virus it would be unlikely to have much effect and by the time a serum exactly corresponding to the epidemic strain could be prepared the outbreak might well be over. Nevertheless the method has possibilities for the protection of key personnel, such as nurses and doctors, during a serious epidemic or major pandemic.

It is not unusual nowadays for sufferers from any ailment to press the doctor for treatment with the well-known "M and B" or with penicillin. Unfortunately none of the virus diseases, with one or two minor exceptions, is amenable to this type of treatment. All the influenza virus

strains so far isolated are completely resistant to both the sulpha drugs and penicillin. This does not mean that a chemical drug effective for influenza will never be found. Research workers all over the world are seeking for new weapons with which to combat the great disease scourges like influenza, and some day a new discovery may relegate all our probings into the possibilities of vaccination and serum treatment to the realm of purely academic interest. Meanwhile, researchers should be encouraged to find out all they can, and patients stricken with influenza should follow the time-honoured dictum—"Go to bed and stay there until the disease cures itself."

Curiosities

You Can Hear Them, Too

THERE used to be a notion going about that fish were deaf. No doubt this idea grew up because the trout in the stream seemed quite unperturbed by the angry shouts of the anglers on the bank, arguing about the size of the one they caught last week. A few years ago Professor von Frisch of Munich showed, however, that the idea was wrong: if a noise was made interesting to the fish they would certainly take notice. One might, for instance, blow a whistle or ring a bell every time they were fed in their tank. Soon they would learn to come to the side for tit-bits whenever the noise was sounded. In this way it was possible to test their range of hearing. There were still a few sceptics, who thought sound waves would not penetrate from air into water, but they were silenced by Professor von Frisch's laboratory assistant who lay under water in the tank with the fish and heard the whistle blow perfectly.

The anatomists then looked into the matter again, and discovered that fishes had a hearing apparatus after all, though no ears in the human sense. But now a new problem was created: what did the fish listen to under natural conditions? And recent investigations have shown that many of them actually "speak," making various kinds of grunting noises. An enterprising biologist in New York has even lowered a microphone under water and recorded what they have to say. Sound, of course, travels very well under water, as anybody who has put his head in the sea, or even under his bath water, can readily testify.

The discovery that fish make characteristic sounds, just like other animals, lends credence to a rather tall but perfectly true story reported by the British anthropologist, Raymond Firth. When the native Malay fishermen put to

sea in their ships, they take with them a highly skilled old man, called roughly, a "jerusalem," whose duty it is, when they have got well off-shore, to dive overboard and listen under water. He is able by picking up the faint sounds that come to him to tell what fish shoals are in the neighbourhood, what sort they are and how many, and so to come up again and direct the fishing fleet. This is a unique method and of course only suited to warm waters. In any case, for North Sea fishing, it is simpler to know the habits of the fish and to set sail straight away for a likely ground. On the other hand, real success this way demands the assistance of the biologist—and that is another story.

The Whale

This is a freak if you like, and one the biologist is still puzzled over. How can a warm-blooded air-breathing animal swim about under water for thirty to sixty minutes and hold its breath all that time? Why does it not suffer from "bends" as a human diver would if he dashed down to 500 fathoms, and back again to the surface? "Bends" is a disease in which bubbles of air appear in the tissues of the body, especially the brain and nervous system, and disorganise them. It occurs whenever the external pressure is rapidly made smaller, as in rising out of deep water, or to a high altitude in the atmosphere; and air dissolved in the blood and fluid of the body is then no longer kept in solution by the pressure but appears as bubbles wherever it happens to be.

Any animal that is 40 feet long or so, and weighs about 150 tons, is bound to be impressive, and difficult for the scientist to handle. It cannot be kept in an aquarium, it is too big to haul into a laboratory (unless the lab. is the size of a Hollywood film studio—and scientists have not got that much money) and to study it at its home means a trip to the Antarctic. Even there, the practical difficulties are by no means over. When a whale dies, immense quantities of sea water pour into its lungs, and ooze into all its tissues,

so that by the time the biologist gets hold of them they are all changed. These changes, partly of decay, are speeded by the fact that the whale is a warm-blooded animal which stays warm for days after death. Its coat of fat, which is between one and two feet thick all over, insulates it from the cold water. This enormous coat of fat, the blubber, is its chief protection against freezing to death. No wonder the infant whale, which is fed at the breast like a human baby, gets a big fat ration in its milk. The human, like the calf, gets about 35 grammes fat per litre of milk (i.e., about an ounce and a third weight in every two pints); whereas the whale has anything from 200 grammes to 400 grammes of fat per litre—about ten times as much. But when it is grown up, the young whale has to rely on itself, and it keeps its enormous bulk alive and working on a vegetarian diet. It swims through the sea with its mouth open, straining off the tiny green plants floating there, many of them visible only through a microscope but nevertheless containing their granules of starch or fat and protein, and vitamins. Does a whale need all the vitamins we do? Nobody knows.

Recently, a peculiarity of the whale's anatomy was discovered which helps to explain how it can hold its breath so long. It has an unusual number of blood vessels apparently specially for storing blood. What happens seems to be this. The whale floats on the surface of the water, breathing away, and the air goes into its lungs and oxygenates the blood flowing through them. But only a fraction of this oxygenated blood is flowing through its tissues and being used up; the remainder is resting in special arterial tanks or sinuses. Now it dives and swims under water, holding its breath. The oxygenated blood begins to flow out of the sinuses through the tissues and is then stored, ready for recharging, in special dilated veins, until the whale comes to the surface again. On arrival it blows all the stale air out of its nostrils, and this it does with a sudden expiration. The sudden release of air pressure

chills the moist air from its lungs, and the "steam" of the spout appears.

Snow

Mid-winter in these islands usually occurs near the beginning of February, and the most likely month of the year for snow is March. One of the reasons for this is the softening, warming influence of the Atlantic, which retains its summer heat much longer than the land. Continental Europe, which has not the benefit of this warm-water system, has snow much earlier and longer, which is the explanation of the snow on Christmas cards, a German custom first introduced to Britain by the Prince Consort about a century ago.

Another important factor in snowfall is the air temperature. During winter, cold air from the icy polar night spreads southwards and meets currents of moist warm air moving north from sub-tropical regions. Whenever they meet, the moisture in the warm air is suddenly chilled, crystallises, and falls. Snow is thus often very localised because it is only formed at the boundary line between air of very different temperatures. It also means that when the air at ground level is well below freezing-point, snow is unlikely to fall, because the cold-warm front must be some distance away. Snow is most likely to fall, in fact, when the temperature is just around freezing point, 32° F.

Fallen snow is only composed to the extent of 10 per cent of water crystals, or flakes: the rest is air trapped in little pockets and channels around and between the flakes, and it is this fact which gives snow two of its interesting properties, those of heat insulation and of absorption of sound. It is well known that air which is unable to move in convection currents is a very good barrier to the spread of heat, and therefore of cold. Clothes keep the body warm because they interpose a barrier of air in the meshes of the fabric, which prevents the dissipation of the heat the body produces. Snow offers the same kind of barrier to

heat spread, which is the reason why Eskimo igloos cut from snow blocks are perfectly warm inside, however the blizzard may rage without.

That snow deadens the noise of footsteps and the sound of shouting is common observation. A year or two ago the National Physical Laboratory, comparing its absorption of sound with that of man-made sound-proofing material, found it to be one of the most effective. A four-inch layer of snow absorbs 90 per cent of all sound of the middle and upper ranges of notes falling upon it, so that echo will be negligible, and a voice which has to carry between snow walls will be inaudible eleven yards away.

Scientific fact and explanation apart, snow crystals are among the most beautiful of natural geometrical forms, and recently a method of preserving their likeness in a permanent form has been found. The freshly-fallen flake is coated with a resin which quickly sets and preserves the crystal's shape when it has melted. A one per cent. solution of polyvinyl formal plastic in ethylene dichloride is used and a drop of this on the flake spreads in a thin film over it and sets as the ethylenic solvent rapidly evaporates. Science thus gives the world a new hobby—snowflake collecting.

"I saw it: I was there"

The eyewitness account of an affair is so often taken for granted as the correct one, that it is well to stop and ask whether this is justified. Leaving aside the existence of optical illusions, and of hallucinations in the mentally abnormal, it is still true that one fails to see everything present in one's field of vision. Who has not searched high and low for a spectacle case or a purse, only eventually to find it in some perfectly obvious and visible place which has somehow been passed over? Awareness of things seen and heard is partly a matter of interest in them, and therefore the degree of attention given to them. This is well seen in animals. The dog sleeps peacefully on the hearth during the crying of the baby, and the clatter of household comings

and goings; but let a distant bark be heard, and at once an ear pricks up. But even with close attention to something it is still possible to miss a great deal. A human face may be very familiar and closely studied, yet the colour of the eyes, the shape of the nose, or the presence of a pimple quite overlooked. One cannot swear evidence on them. The reason for this seems to lie in a lack in the observer of a system on which to build his observations. He has no scheme on which to analyse what he sees, he does not know the special points to look for, and consequently he gains merely an overall impression and not a detailed knowledge. It is the purpose of training to develop a scheme of analysis. The policeman is told to pay particular attention to eye-colour, the presence of scars, and so on, and to ignore whether the eyes are beautiful or not. In this way he is able to make a group of accurate observations for identification—and this is the purpose of scientific training in general, to narrow the field of observation in special ways for special ends, and by concentrating on the relevant to learn a great deal more about it than is evident to the casual onlooker. The conclusion from this is that the professional eyewitness is the most reliable. It is only the *trained* onlooker who sees most of the game.

All this is common experience. But scientists have been interested to find by experiment exactly how unreliable the eyewitness is. Professor Stern of Berlin was interested for legal reasons, and used to hold discussions in which previously arranged violent quarrels would break out. Legal actions for assault would be expected, so he would announce afterwards, and he would ask other members of his discussion circle to write down accounts of the quarrel as they saw it, as possible witnesses if a court case actually developed. These accounts showed conclusively that people were quite unable to note what went on in the heat of the moment.

More recently, in 1932 under the auspices of the Society for Psychical Research, Theodore Besterman investigated

how far those present at a spiritualistic séance would be likely to know what was really going on. He held six "sittings" under identical conditions with a friend playing the part of medium. At each sitting a different group of seven people, some of whom were spiritualists and some not, were told that an experiment was being held to find out how far the eyewitness was reliable in an account of a séance, and that they should therefore watch carefully what happened, taking notes if they wished, and would afterwards be questioned on it. Besterman then went through the same routine to an exact timetable at each sitting, lasting twenty-five minutes in all. Most of the time the room was lit, though not very brightly, and for periods the gramophone was played. After one of these musical interludes, a knock was heard on the door, whereupon the "medium" moved a bell from one end of the table in front of her to the other, and Besterman got up, went to the door, opened it, went outside and spoke to someone and then returned to his seat putting a white card in his pocket. Later, the lights were turned out, the "medium" moved the bell back to its original position, stuck a drum-stick in the curtains behind her, and pinned a cloth over her face. After due warning, the lights were flashed on again for a moment only. At the twenty-fifth minute, they were turned full on for good, and the observers asked to answer a question-paper on the proceedings.

These questions were of various kinds, for instance how many people were present at the séance (which nearly half those present got wrong—no doubt forgetting to count themselves in). When asked what they noticed during the early disturbance (the knock on the door), eleven out of the 42 had nothing to say, and of the remaining 31, five did not refer to Besterman going to the door, and 13 were unaware that the door was opened and that he left the room. Of course there are several possible explanations for this, such as, that Besterman being the organiser nothing he did was to be counted in as part of the séance and watched closely,

or, that the events were too commonplace to be interesting, and were ignored in the absence of instructions to the contrary.

But what is harder to explain away, is the fact that 13 of the 42 people had "illusions" of one kind and another, fancying things which did not occur. One reported hearing "two raps on the ceiling, the first louder than the second, both metallic in quality." Another saw a small narrow vertical light hanging for some time in the air during the period of darkness. Another claimed that during this time a xylophone on the table glowed with a pink colour. Several people thought that the bell on the table was a glass bottle or a glass of water, which later appeared "opaque and like a handbell," or was actually replaced by a bell. It is this sort of thing which casts doubt, not on the veracity, but on the accuracy of the eyewitness, and makes reports of psychical manifestations of somewhat dubious value.

Part of the difficulty seems to arise, not in observing, but in remembering afterwards. Professor Bartlett has suggested that we only really recollect the broad framework of an incident, and invent what we regard as "suitable and probable detail" to clothe it when we come to tell what happened. If this is so, the importance of good cross-examination of witnesses in court becomes apparent. Counsel must discover how much of their evidence is merely suitable and probable detail.

Chains

ALAN ROBERTSON

TYPHOID has become a disease of such rarity in this country that, when cases do occur, they have sufficient news value to make immediate appearance in the daily press. In many places overseas, however, the disease is fairly widespread and it is occasionally reintroduced to Britain by travellers returning from abroad. If such a person comes back to London, say, without knowing that he is at the time infectious, he may meet and infect several people before it is realised that he has typhoid. But he has started a chain which may carry on until a great number of people have the disease. In fact, he may start an epidemic. That is, the number of persons affected may continue to increase rapidly until the gravity of the situation is realised and steps are taken by the medical authorities to cut down the spread of infection. Or he may infect only one person, who infects no others, and the chain end there. The crucial factor is the average number of people infected by those carrying the disease before they are recognised as carriers and isolated. If this factor is greater than one, there will be an epidemic; if it is less than one, there will not. In the source of infection overseas, the factor is probably fluctuating above and below unity depending on the season of the year; and the time lag in the development of the disease means that there is always some present in the population. Then it is said to be "endemic."

Still, why this talk of typhoid in an article which is obviously about chemistry? It so happens that a class of very important chemical reactions are indeed formally similar to such a process as the spread of an infectious disease. They have in fact been given the name, chain reactions. Dr. Eley touched on the subject briefly in his

article on reaction speeds ("Not Too Fast and Not Too Slow") in the first issue of *Science News*. Before going into more detail about the many chain reactions which are of practical importance, it may make things clearer to take one particular reaction, and explain the various terms used by reference to the typhoid analogy.

Any elementary textbook of chemistry will tell you that two volumes of hydrogen and one volume of oxygen combine together to give two volumes of steam. But that gives no more idea of how the individual molecules take part in the reaction, than to say that Charles II reigned from 1660 to 1685 and expect your listeners to understand that that includes the Plague, and the Fire, and Nell Gwyn and so many others. Let us write down the reaction as the chemist does:

$$2H_2+O_2 \rightarrow 2H_2O$$

It looks quite simple—two molecules of hydrogen and one molecule of oxygen. But actually such isolated reactions between three molecules rarely happen. They require the simultaneous meeting of the three molecules in just the right configurations and, as molecules in a gas are dashing about at high speed, such a happening is rare. In fact, in the dark, hydrogen and oxygen react only very slowly. But, under the influence of light or an electric spark, the mixture may explode. The light or the spark, which we call the chain initiator, resembles the initial person who had typhoid. Before he came along, all the molecules reacted slowly (or if you like, died of old age) but immediately he appears the reaction proceeds like an epidemic. Thus one chain initiation may cause the formation of many thousand molecules of H_2O. In an epidemic, the timely isolation of an infected person may save many people from catching the disease. Similarly, the removal of one "chain carrier" from the reaction can stop the formation of many water molecules.

The reaction of hydrogen and oxygen, then, is not one single reaction but a sequence of reactions. The electric

spark which starts the ball rolling, fragmentates some of the gas molecules, and these fragments, either single atoms or the incomplete molecules known as radicals, react amongst themselves, the final overall product left at the end being water.

There is one very important characteristic of chain reactions—their great sensitivity to small quantities of impurity. Thus a small concentration of iodine is sufficient to cut down the rate of the hydrogen-oxygen reaction very markedly. Such chemical compounds which slow down the reaction are called "inhibitors." In the absence of an inhibitor, the chains may be stopped by an internal change in the carrier, by its absorption on the walls of the reactant vessel or by mutual reaction of two carriers, as for instance two H atoms in the hydrogen-oxygen reaction, to give a hydrogen molecule.

$$H + H \rightarrow H_2.$$

A chain carrier may either react to further the chain or become sidetracked by one of the above processes without producing any "offspring." The factor which is important is the average number of offspring produced per lifetime (called in nuclear physics the multiplication factor, which we will use here). If this can become greater than one, then the reaction can become explosive. It can in fact only become greater than one if in some of the chain propagation reactions, two or more carriers are produced for one, such as

$$O + H_2 \rightarrow OH + H$$

in the H_2–O_2 reaction. This is known as chain branching. If we were to draw a pedigree of the reaction, it would be branched like a tree—if there were no chain-branching, the pedigree would be a straight line. A further important quantity we shall have to refer to is the "chain length," which explains itself. In a given reaction all chains will not be the same length—some may be stopped after ten steps and some after a thousand—and we can only refer to the "average chain length."

It is possible to press the analogy of the infectious disease

rather further. In the case of typhoid many cases are known of persons who can infect others with the disease while being completely unaffected themselves. They are known as "typhoid carriers." In the chain reaction, the analogue is the "catalyst" which causes many molecules to react while remaining unchanged itself. For instance, the addition of the enzyme, catalase, to a solution of hydrogen peroxide under given conditions, causes 44,000 molecules to decompose every second for each molecule of catalase present. Although in the older literature it was stressed that most catalysts which act in this way are unchanged, it now seems very probable that they actually take part in a cycle of reactions, returning to their original state at the end.

What is the nature of this chemical infection? In general, it is excess energy (either chemical or heat energy) which cannot spread itself throughout the reacting mixture as a whole as energy normally does. For instance, a hydrogen atom in the hydrogen-oxygen reaction may satisfy itself by reacting with one of the gas molecules, but only at the expense of releasing other atoms or radicals:

$H+O_2 \rightarrow O+OH$ (oxygen atom and hydroxyl radical)
or $H+H_2 \rightarrow H_2+H$

and so on. In some cases, the energy is heat energy which before it has time to diffuse away from its source has caused other molecules to react.

We can now sum up the position by means of our analogy.

Disease	—corresponds to—	*Reaction*
Introduction of infection		External chain initiation
Disease carrier	,,	Catalysis
Spread of the disease	,,	Chain propagation
Recovery without infecting anyone else	,,	Chain termination
Isolation	,,	Termination by inhibition

We can then write the whole reaction as follows:

Chain initiation $H_2 \rightarrow H + H$
$O_2 \rightarrow O + O$
Chain propagation $OH + H_2 \rightarrow H_2O + H$
$O + H_2 \rightarrow OH + H$—chain branching
Chain termination $H + H \rightarrow H_2$
$H + OH \rightarrow H_2O$
$O + O \rightarrow O_2$
or absorption on vessel walls.

Even this is a simplified picture, because there is the possibility of the formation of hydrogen peroxide H_2O_2 by reactions such as $OH + OH \rightarrow H_2O_2$.

Chain Processes in Nuclear Reactions

The whole conception of an atomic bomb rests upon the fact, discovered in 1939 by Meitner, that after the capture of a neutron by U-235, the uranium nucleus splits up to give two nuclei of about half the atomic weight of uranium (with consequent loss of mass and therefore production of energy) and also between one and three fast neutrons which can repeat the process on other uranium nuclei. U-235 is peculiar in that, in its case, fission is caused by neutrons of "thermal energy," i.e., neutrons which have a stable energy value corresponding to the normal energy of movement of a gas molecule at that temperature, whereas most other nuclei of similar size require high energy neutrons to cause fission and will not absorb thermal neutrons. An exception, of course, is Plutonium-239 which behaves in a similar way to U-235.

Consider the introduction of a neutron into a mass of U-235. If it has energy intermediate between the high energy fission level and the thermal fission level, it will gradually lose this by collision with the nuclei until it reaches thermal energy levels. There it is absorbed by a U-235 nucleus which immediately splits up to give the above fission products and *more than one* neutron of high energy. Here we have the first important necessity—chain branching. The neutrons either

(a) are absorbed with high energy to cause fission;
(b) are degraded in energy to thermal levels and thus cause fission;
(c) escape from the whole mass;
(d) are absorbed by impurities or non-fission capture by uranium.

Now in order to get a final explosion, it is necessary that a neutron should produce more than one neutron in the next generation. Even if it only produces 1.005, say, on the average, that will be sufficient—the important fact is that the figure should be greater than one. It is therefore necessary to cut down as much as possible the losses from (c) and (d). The absorption by impurities can quite obviously be tackled by removing the impurities—simple enough in statement but somewhat more difficult in practice. The loss through escape can be decreased by increasing the whole mass of uranium. There are proportionately more people in U.S.A. who have never seen the sea than there are in Great Britain. So it comes about that above one particular size, the multiplication factor becomes greater than one and the whole mass disintegrates. The critical size can actually be lessened somewhat by using a "tamper" around the uranium to reflect some of the escaping neutrons back inside again. In the atomic bomb cosmic rays and chance disintegrations are relied upon to produce the all-important chain-starting neutrons. This means that it is absolutely impossible to keep together a mass of U-235 greater than the critical size.

It was mentioned above that Pu-239 reacted with thermal neutrons in a similar way to U-235. Pu-239 is formed by a neutron of energy within a certain range meeting an atom of U-238 which then emits two electrons in succession to give Neptunium-239 and Plutonium-239 respectively. If therefore U-238 could be exposed to a source of neutrons, Pu-239 would be formed which could be separated from uranium by purely chemical means. The obvious source of neutrons was in the fission of U-235 after slow neutron

capture. Therefore it would seem possible to set up a plant to produce Pu-239 from natural uranium (containing 0.7% of U-235 and 99.3% of U-238). The problem here is to get a multiplication factor of slightly greater than one and then, when the reaction is going at a reasonable rate, keep the rate steady by reducing the multiplication factor to exactly one. The same problems of critical size were met as in the atomic bomb. The question of how to construct a reacting mass of much larger size while only using a small amount of uranium (as there was very little available) was met by the use of a moderator of similar function to that of the tamper in the bomb. The atomic pile was therefore a framework of uranium rods with graphite blocks in between. The graphite does not absorb neutrons but reduces them to thermal energy levels very quickly. Thus the overall leakage out of the sides of the pile is reduced without increasing the amount of uranium present. The multiplication factor could be controlled by the insertion or withdrawal of bars of cadmium metal which are exceptionally good neutron absorbers. In order to minimise the size of the pile, the graphite had to be exceptionally pure in order to reduce absorption losses by impurities. Once again the chain starters were contributed by cosmic rays or chance spontaneous disintegrations of the uranium.

Gas Fires and Aircraft Engines

We started off by using the reaction between hydrogen and oxygen—or the "combustion" of hydrogen—as an example of a chain reaction. This reaction has been looked into very thoroughly in the laboratories, for it is the simplest of the many reactions upon which the internal combustion engine depends. And when we consider the complexity of the simplest reaction, we may well be taken aback by the problems of the combustion of a complex hydrocarbon, or rather a mixture of complex hydrocarbons, which is what we have to deal with in the petrol engine.

We mentioned before that the reaction chains in the

hydrogen-oxygen reaction may be stopped on the surface of the containing vessel. The relation of surface to volume has therefore a considerable effect on the multiplication factor and therefore on the type of reaction. Suppose we take mixtures of hydrogen and oxygen in the right proportions (2 : 1) and let them react at different known temperatures and pressures. The results we get can be described most easily by the diagram:

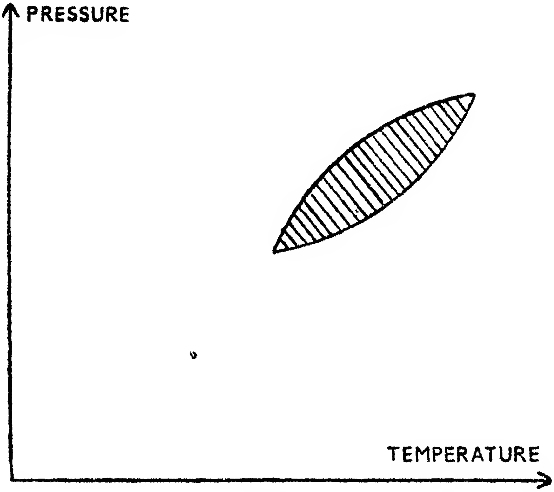

Figure 13

If the conditions lie inside the shaded area, the mixture explodes—if not, we have a steady reaction. It all depends on whether or not the multiplication factor is greater than one. At low pressures, the chain carriers have a fair chance of getting to the walls of the vessel before they react. This is rather similar to the neutrons in the atom bomb escaping from the uranium. As the pressure is raised, the chain carriers meet more gas molecules before they get to the walls and therefore have a higher chance of reacting and

carrying on the chain. Then as the multiplication factor becomes greater than one, there is a sudden transition from a slow reaction to an explosion. This explanation is borne out by the fact that the lower limit can be altered by changing the surface of the vessel.

As the pressure is raised more and more, the multiplication factor begins to drop once more because there are so many chain carriers present that they react with each other and cancel each other out. We get another transition from an explosion to a slow reaction which cannot be affected by changing the surface conditions.

The reaction is very susceptible to the addition of small quantities of other gases of which nitrogen peroxide, NO_2, is the most interesting. Add a little NO_2 to a slowly reacting mixture, little happens, the reaction goes a little faster—add some more, explosion—add more still, a steady reaction once more. The NO_2 can apparently function both as a chain initiator and as a chain stopper, probably by reactions similar to the following:

$NO_2 \rightarrow NO + O$ (initiating chains by producing an oxygen atom)

and $NO_2 + H \rightarrow HNO_2$ (stopping them by removing a hydrogen atom).

On the other hand, chlorine and iodine have only the property of stopping chains by a reaction such as

$$H + Cl_2 \rightarrow HCl + Cl$$

the chlorine atom being apparently fairly unreactive.

This is actually only half the story—it is also known that we can get hydrogen peroxide, H_2O_2, formed in the process and that this by its decomposition can start further reaction chains by means of the radical O_2H. The research chemist has then to proceed to build a reaction scheme for the whole process in terms of simple unit reactions. It is rather like being given a picture of St. Paul's Cathedral and told to build a copy. You can only guess what the bricks are made of and how they fit. The atoms and radicals which are taking part in the reaction are probably present in very

small concentrations and when the reaction is stopped, they all immediately disappear. It is therefore often possible to construct several schemes which produce the same answer from the same starting point, but which are completely different in between. Occasionally, it is possible to get confirmatory evidence by means of measurements of light absorption—for instance, it has been shown that the CH radical is present in the blue flame of the Bunsen burner and that formaldehyde HCHO has much to do with knock in petrol engines. Quite often, however, we reach the answer that your guess is as good as mine and that's an end of it.

In the hydrogen-oxygen reaction, we can only postulate four possible chain carriers—O and H atoms and OH and O_2H radicals. If we now pass to methane, CH_4, the possibilities are many, and this is only the simplest of the hydrocarbon series. The combustion of hydrocarbons shows similar phenomena to that of hydrogen in the existence of explosion zones and great sensitivity to small quantities of impurities. From the practical point of view the most important phenomenon is the occurrence of "knock" in ordinary petrol engines. It can be shown theoretically that the efficiency of an internal combustion engine depends, amongst other things, on the compression ratio in the engine. This is the ratio of the final pressure in the cylinder, when the petrol-air mixture is compressed, to the initial intake pressure. It has been found that petrol mixtures are subject to "knock" at high compression ratios and this effectually limits the efficiency of the engine. "Knock" is actually the explosive reaction of part of the gas mixture, causing pressure waves to be reflected back and forwards throughout the cylinder. Different hydrocarbons have different knock tendencies, i.e., knock sets in at different compression ratios. An arbitrary scale has been introduced, to rate any fuel, known as the "octane number." This essentially gives the mixture of two pure hydrocarbons (normal heptane and iso-octane) which has the same knock

tendency as the given fuel. Thus to say that a certain petrol is "87 octane" means that it has the same knock tendency as a mixture of 13% normal heptane and 87% iso-octane. Actually, modern techniques of petrol refining have reached such a pitch that octane numbers of over 100 are quite normal for military aircraft engines. The compression ratio of an engine is of course governed by its design and the minimum octane number of the petrol to be used in an aircraft engine is always specified on it. It does no harm to the engine to use petrol of a high octane number in an engine only requiring a fuel of lower rating. It is merely using good petrol where poorer would have done.

The phenomenon of knock is still far from completely understood. As far as is known at present, the first product of the combustion reaction is a peroxide which is formed by a free radical reaction—a free radical is essentially an incomplete molecule as in the following series:

CH_4	CH_3	CH_3OOH
methane	methyl radical	methyl peroxide

The peroxides then decompose to start further reaction chains. The phenomenon of knock has been correlated, by means of light absorption measurements on the reacting mixtures, with the presence of formaldehyde, HCHO. It seems that the reaction of formaldehyde with the peroxide provides the chain branching reaction which is necessary for an explosion to take place. It is possible to add various substances to the petrol to reduce the knock tendency and therefore increase the octane rating. The function of these is to react with either the formaldehyde or the peroxides. Aniline is a standard case of the former type of "anti-knock," and lead tetra-ethyl $Pb(C_2H_5)_4$ of the latter. It is known that it is not lead tetraethyl itself which is effective but its decomposition products, either small particles of lead or lead dioxide.

The whole problem of knock ratings of fuels has become much less pressing with the development of the jet engine which worries little about the type of fuel which is fed

into it, provided that it is of fairly low molecular weight.

Another problem in the aero engine and in the car engine is the deterioration of the lubricating oil. The oil in its passage around an aircraft engine has an average temperature of about 100° C. and under these conditions absorbs oxygen from the air to give, in the end, organic acids which corrode the bearings. A fair amount of work has been done on this problem from the theoretical aspect in recent years, and here again, as in the case of knock, it seems that peroxides are the villains of the piece. From the research aspect, many interesting points arise in the reaction by which the peroxides are formed. This reaction is very readily accelerated by some metal soaps (such as copper, cobalt and iron stearates) and also much affected by small quantities of inhibitors (such as phenols, aniline, etc.). In the reaction without addition of metallic catalysts, we have the odd fact that the rate of reaction is independent of the oxygen pressure. In this case, inhibitors (actually β-naphthol) have been used to give an insight into the mechanism of the reaction. If we add a certain quantity of inhibitor and determine the rate at which it is used up, this gives us the rate at which chains are being started in the reaction and by comparison with the overall reaction rate, also the chain length of the reaction, which turns out to be several hundreds. Argument still goes on as to the actual nature of the chain intermediates. The metal-catalysed reaction is interesting in that above a certain minimum concentration, further addition of catalyst has little effect on the reaction rate. It would seem that the catalyst plays three parts in the reaction—starts chains, stops chains and decomposes the peroxide formed. The number of inhibitors suggested for use in lubricating oils must run into thousands; not that there are a thousand different types, but a thousand modifications of a few types to get round other firms' patent specifications. In a catalysed reaction, of course, the inhibitor, as well as merely stopping chains, can perform quite effectively just by reacting with the catalyst.

The absorption of oxygen from the air has been granted a special name, "autoxidation." The first substance thoroughly investigated was benzaldehyde which is very rapidly oxidised at ordinary temperatures to benzoic acid.

$$C_6H_5\,CHO \rightarrow C_6H_5\,COOH$$

It was found that this reaction was inhibited by alcohols such as isopropyl alcohol $CH_3\,CH(OH)\,CH_3$ and what is more, it was found that in the process the alcohol was oxidised to the corresponding ketone, although it will not, by itself, react with oxygen. This is known as "induced oxidation." Other interesting autoxidation reactions are the oxidation of chloroform to phosgene, a poison gas, and the oxidation of unsaturated organic molecules such as rubber and linoleic acid. The autoxidation of chloroform is quite simply stopped by adding ethyl alcohol as an inhibitor. The oxidation of rubber is exceptionally important in that this is the major factor in the perishing of natural rubber. The attack by oxygen leads to a breakdown of the long hydrocarbon chains and a consequent loss of springiness. The oxidation of unsaturated fatty acids is very important in the setting of paints. The oxidation is here accompanied by a polymerisation and the film of paint gradually becomes solid. Commercially quick drying paints normally contain some cobalt linoleate to make the oxidation and therefore the polymerisation go faster.

Photochemical Reactions

It is obvious that in many reaction systems, there are chain reactions which lack only a chain initiator to give them the word "go." A possible source of initiation lies in the absorption of a quantum of light. For instance, if an oxygen molecule in a mixture of hydrogen and oxygen absorbs a quantum of light and is split into two O atoms, the initial reaction may cause the formation of several thousand molecules of water. This presents a very powerful tool to the investigator. He can measure the extent of reaction caused and also the number of quanta of light

absorbed. The chain length of the reaction can thus be worked out and other details elucidated. For instance, if the chains end by collision of two chain carriers, the rate of reaction will be proportional to the square root of the rate of light absorption, and so on.

Real Chains This Time

Molecules with a double bond in them, like ethylene, will react with a hydrogen atom, or in fact any "free radical," to give another free radical. When the double bond breaks one end is satisfied and the other isn't. We can write this as follows:

$$H + \underset{\text{ethylene}}{CH_2{=}CH_2} \rightarrow CH_3{-}CH_2{-} \text{ (an ethyl radical)}$$

and then the following reaction can happen:

$$CH_3{-}CH_2{-} + CH_2{=}CH_2 \rightarrow CH_3{-}CH_2{-}CH_2{-}CH_2{-}$$

Now this is just the same type of reaction as we have had before, with the difference that the chain reaction gives rise to a material chain consisting of many units or links of ethylene molecules. This is important because rubber is just such a long chain-like molecule or "polymer." So that if we can build up such chains we can produce substances with rubber-like properties.

The repeating unit in the natural rubber chain is an isoprene molecule

$$\begin{array}{c} CH_3\ \ H \\ \ |\ \ \ \ \ | \\ -CH_2{-}C{=}C{-}CH_2{-} \end{array}$$

The great practical disadvantage of such a molecule, itself containing double bonds, is that it is very liable to attack by the oxygen of the air, causing "perishing." The numerous synthetic plastics which can to some extent replace rubber are, in general, more resistant to oxidation although they may not be so good from other points of view. The units commonly used in building up the synthetic chains are as follows:

$—CH_2—CHCl—$	polyvinyl chloride
$—CH_2—CH(C_6H_5)—$	polystyrene
$—CH_2—CH_2—$	polythene
$—CH_2—CH(COOCH_3)—$	polymethyl methacrylate or Perspex.

The rubber-like properties which they show are connected with the capacity of the long chain of atoms to coil itself up in a ball needing tension to pull it out.

The reactions by which the polymers are made follow the normal rules of chain reactions. In general, a catalyst is used to start off the chains by producing free radicals, by processes such as direct decomposition of an unstable substance like benzoyl peroxide (used in the manufacture of polystyrene) which decomposes as follows:

$$(C_6H_5CO)_2O_2 \rightarrow C_6H_5— + CO_2 + C_6H_5CO_2—$$

or by causing the decomposition of hydrogen peroxide by adding a catalyst such as iron to it. Hydroxyl radicals are produced by the following reaction:

$$Fe^{++} + H_2O_2 \rightarrow Fe^{+++} + OH + OH—$$

It is interesting that both the above methods of starting chains have been thoroughly investigated in the laboratory and it has been shown definitely that the free radical which starts the chain reaction does in fact become incorporated in the molecular chain.

Another type of polymerisation which is of great theoretical and practical interest is the so-called explosive polymerisation of substances similar to ethylene which, oddly enough, will only occur at low temperatures such as —80° C. They are catalysed by aluminium trichloride and it seems that a trace of water is necessary to make the reaction go at all. They are exceptionally difficult reactions to investigate because of the difficulties of doing experiments at —80° C. and also because of their very high speed. It is thought, however, that they are ionic reactions—that is, that the first step in the chain is the addition of a hydrogen

ion rather than a hydrogen atom to the compound containing the double bond.

The fact that the chains actually exist as chains of atoms makes the investigation of polymerisation reactions somewhat simpler than other chain reactions. The average chain length can now be measured by finding the average molecular weight of the polymer formed. In some cases it has been possible to measure the length of the chain in time. Results vary from one-hundredth of a second to one minute for chain lengths of the order of thousands.

It is fascinating that the same chains of thought can be applied to infectious diseases and atom bombs and polymers. The same ideas also apply to the growth of populations—a family tree is formally the same as a chain branching reaction. Here however the problem is complicated because it takes two in one generation to produce offspring in the next, and the ability to reproduce depends on a person's age. To deal with population growth the "Malthusian parameter" was evolved in the early part of last century. It is actually equivalent to our multiplication factor minus one. The fall off in reproductive ability with age masks the effect of the multiplication factor and it is possible to have the latter less than one while the population is still increasing in number. Nevertheless, the number in the population capable of reproducing is falling, though at present outweighed by an increase in the number of old people. This has been the situation in this country for the past twenty years. It is due to such a time lag in the effect of the altered multiplication factor that it is possible to estimate the population so far ahead.

In this way the most diverse phenomena in the universe can receive the same formal mathematical treatment. Though they are qualitatively very different, these processes can all be understood through the same mental approach, as we have very briefly sketched.

*The Microflora of the Alimentary Canal**

SIR JOSEPH BARCROFT, F.R.S.

It is perhaps not generally realised that the alimentary canal harbours a vast population of living organisms. These may be injurious to the host, as, for instance, worms, or they may be beneficial, or, so far as is known, they may be neither. So varied are their forms that to give an account of them would be something like describing a census and would be far outside the scope of this article. Here I am dealing merely with the forms that are held to be beneficial and the emphasis will be upon the ways in which they are believed to perform their beneficent functions, rather than upon the organisms themselves. They are not uniformly distributed down the alimentary canal, but function in those parts of the alimentary canal in which the food is retained for a considerable time, that is to say in the large intestine of mammals generally and in the rumen of ruminants.

The organisms of which I speak are for the most part unicellular and may be divided into what are regarded technically as being within the animal kingdom and those which are regarded as being within the vegetable kingdom; put shortly, the alimentary canal harbours a population of microfauna and a population of microflora.

The importance of this population of microflora to the digestive processes and maintenance of the animal varies in different species. A focal point in all mammals is the large intestine. Let me say at once, though the detailed proof will appear later, that the intensity of bacterial digestion seems to be much the same in the large intestine of all

* Based on lectures given to the Royal Society of Edinburgh, and the Natural Sciences Society in Copenhagen.

mammalian species so far investigated, sheep, rabbit, pig, dog and horse. The scale on which it takes place is very different in different species, varying with the size of the large intestine itself.

In the dog, endued as it is with a very small large intestine, microbial digestion is an almost negligible affair; in the horse, on the other hand, in which the large intestine is enormously developed, bacterial digestion takes place on an important scale. This is in keeping with the diets of the two animals, for the dog being a carnivore very little food remains undigested by the time its intestinal contents reach the caecum; the horse, on the other hand, eats a diet in which cellulose looms large and for which provision for bacterial digestion is important.

In the ruminants the food is subjected to the action of bacteria before it meets the other digestive juices. The bacteria, therefore, have additional opportunities. They have the opportunity of attacking the whole diet of the animal and of digesting, besides cellulose, such other carbohydrates as are amenable to their action. Not that carbohydrate digestion is complete before the food leaves the rumen; merely there is an opportunity of its taking place therein.

The functions attributed to the flora of the alimentary canal fall into three categories:

I. Those related to vitamin synthesis.

II. Those related to the digestion of carbohydrates, the general idea being that they convert carbohydrates into volatile fatty acids, which, in turn, can be used as sources of energy.

III. Those related to protein formation, the general idea being that the microflora form a basis of food for the microfauna and that the microfauna form a source of protein of the animal.

Naturally, the rôles attributable to the microfauna and microflora respectively may differ in kind in alimentary canals of different species and certainly differ in degree.

I. The Microflora considered in relation to Vitamin Synthesis

It is usually held that animals cannot synthesise vitamins and therefore that animals depend for their vitamins on the vegetable food which they eat. To this general thesis farm animals seem to stand in an exceptional relation, for whilst they obey the law as strictly stated, they carry about the necessary vegetables in their alimentary canals, and therefore the cow or sheep as she stands is largely independent of vitamins in the food which she eats.

This conception was, I believe, first put forward by Theiler, Green and Viljoen, working at Onderstepoort, who said, with reference to early experiments on Vitamin B: "It may be that cattle are capable of synthesising their own vitamins in virtue of the extensive bacterial flora of their intestinal tract."

The suggestion was put forward merely as a possible explanation of the failure on the part of the authors to produce avitaminosis in cattle which were fed on vitamin deficient diets. Apart from the demonstration by Scheunert and Schieblich (1923) that symptoms of Vitamin B deficiency could be warded off in rats by administration of cultures of bacillus vulgaris grown in media free from Vitamin B, the first real proof was offered by McElroy and Goss in 1939, by which time, of course, we no longer talked in terms of Vitamin B as did Theiler, Green and Viljoen, but in terms of the Vitamin B complex: in terms, that is to say, of thiamine, riboflavin, pantothenic acid, B_6 and nicotinic acid.

Working on sheep they obtained the following comparison between the members of the complex in the supposedly deficient ration and in the dried rumen content.

	Per Gram of Ration	Per Gram of dried Rumen Content
Thiamine	< 0.4 micrograms	> 5 micrograms
Riboflavin	< 0.3 micrograms	> 20 micrograms
Pantothenic Acid	< 0.2 chick units	5 chick units
B_6	< 1.5 micrograms	6-10 micrograms

The same authors attacked the subject another way and performed the following experiment. They fed a cow for $4\frac{1}{2}$ months on diet approximately free from members of the Vitamin B complex, thiamine, riboflavin, pantothenic acid and B_6. The cow after 80 days on this diet gave birth to a calf which was itself fed for 43 days solely on its mother's milk. In that time it put on weight at an average of 1.7 lb. per day. Tests of the rumen content of the cow and of the milk demonstrated satisfactory quantities of Vitamin B.

This result was not controlled by an experiment in which an animal was fed on the same diet, but in which the alimentary canal was kept free from micro-organisms. I do not know whether it would be possible to carry out such an experiment. Further research has been in the direction of dotting the i's and crossing, or even crossing out, some t's, but fundamentally the work of McElroy and Goss stands.

So much for cattle, but they are not the only animals in which vitamins appear to be formed in the alimentary canal by microflora.

Rats have been known to thrive on a diet approximately free from water soluble vitamins. Unlike the sheep they have no rumen in which microflora can synthesise the material; the only incubation chamber which they possess is the large intestine. By the time the food has reached the large intestine, however, it has already passed the principal site at which products are taken up from the gut by the blood, namely, the small intestine. The rat, however, seems to have the power of absorbing Vitamin B from its own fæces and even eats them, thus husbanding the vitamin manufactured in its large intestine. This process was first recorded by Fridericia (1926) and called by him "refection." Curiously, it only happens when indigestible bulky diets are given, such as uncooked potato starch. Phases of this observation have been confirmed by Scheunert and Schieblich (1923), Roscoe (1927), Kon and Watchhorn (1927), Bliss and Green (1936), Burkholder and McVeigh (1942), and many others.

II. The microbial conversion of carbohydrates into fatty acids

I have taken roles II and III in that order, because ultimately we shall be faced with their relative importance. That is to some extent a matter of comparing the scale on which each takes place. We are not in a strong position to do that, but we know more, quantitatively, about the scale on which microbial conversion of carbohydrates into fatty acids can take place, than we do about the scale on which protein is formed as the results of protein synthesis by bacteria and its ultimate conversion into a diet for the sheep through the medium of ciliates.

The idea that cellulose can be converted in part into volatile fatty acids is no new one. Anyone to-day who reads a paper published more than sixty years ago by Tappeiner (1884) may well, as the phrase goes "rub his eyes."

To the historian of science it must always be a matter of interest to assess just when a discovery may be said to have taken place. Many a great and convincing discovery has been made, and when made it has been found possible to look back into the literature and find that the observation is not new. It has already been made; but it has failed to convince the scientific world of its day; it has been forgotten; it is then re-made in a convincing way and its importance is recognised. So with Tappeiner. He showed quite clearly that in an artificial digestion *in vitro* acetic acid could be made out of cotton wool, by inoculating the suspension with rumen contents. But the scientists of his day discounted the importance of this observation, focussing attention on the rôle of cellulose as roughage, and so the position remained till the work of Woodman and Evans revived an interest in the breakdown of cellulose as effected by fermentation ?

The Course of Microbial Digestion of Carbohydrates

The course of microbial digestion of carbohydrates may be studied in the actual animal or *in vitro*. In fact, it should

be studied in both ways with constant comparison of the two methods. Qualitatively the same results are obtained, which are, that cellulose, starch and glucose are converted first into lactic acid and later into volatile fatty acids.

The next step was made by Phillipson (1942) who carried out a survey of ruminant digestion in sheep. Three important points emerged from this survey:

(1) That soon after the introduction of a meal, especially if the meal consisted of mangolds or other materials rich in sugar, the contents of the rumen underwent a considerable rise in acidity.

(2) That this rise was due to the production of lactic acid and volatile fatty acids, the presence of lactic acid being of an evanescent character, and

(3) The probability that the products of digestion could be absorbed from the rumen itself, which supposition was a revolt against the then orthodox doctrine that these bodies were not absorbed until they reached the intestine.

It was at this juncture that the Agricultural Research Council set up the Unit of Animal Physiology at Cambridge, which gave the opportunity of balancing the staff by the addition of a microbiologist, Dr. Elsden, to the team consisting of Dr. Phillipson and Miss McAnally. Dr. Elsden had indeed started research in Edinburgh upon the digestion of cellulose *in vitro* by the inoculation of suspensions of filter paper with rumen content. The field was ready for an attack on a considerable scale. Among the first questions which arose were:

(1) Can anything be absorbed from the rumen?

(2) *If so, what is absorbed from the rumen?*

(3) Under what circumstances does absorption take place?

(4) What absorbable materials are formed in the rumen?

I will endeavour briefly to reply to these questions.

(1) *Can anything be absorbed from the rumen?*

Ocular demonstration of the fact of absorption through

the stratified epithelium of the rumen can be furnished in the following way. If every outlet from the rumen be tied off and 500 c.c. of a solution of sodium ortho-iodo-hippurate be inserted into it (after the manner in which the urologists use 'uroselectan') after five hours an X-ray shows that the opaque material has almost entirely left the rumen and is found on the other hand in the bladder. Moreover, the bladder contents are found to be rich in iodine. If a substance with a molecular weight of 327 is known to traverse the wall of the rumen, no great intellectual strain is being presented by the hypothesis that acetic or propionic acid can do the same (molecular weights 60 and 74).

(2) *If so, what is absorbed from the rumen?*

Anticipating the answer to question 4, let me say at once that the blood coming from the rumen veins during digestion contains quantities of acetic and propionic acids of quite a different order from the quantities found in the general circulation. When we come to consider the blood flowing from different parts of the rumen, we find that the volatile acids occur in much greater quantity in the blood from the anterior vein than the posterior. The anterior vein drains that part of the organ which is intimately connected with the material undergoing digestion, whilst much of the part drained by the posterior vein, in the position in which we operated, was in contact with gas.

TABLE I

Concentration of Volatile Acids in Blood from the Rumen and from the General Circulation respectively.
(c.c. N/100 volatile acid per 100 c.c. blood)

Experiment	*Carotid Artery*	*Jugular Vein*	*Rumen Veins* *Posterior*	*Anterior*
1	5	0	38	—
2	2	1	31	—
3	4	5	21	—
4	4	4	22	—
5	2	4	30	—
6	4	4	20	—
7	12	13	35	—
8	—	—	17	51
[illegible]	—	—	41	50

Experiment	Carotid Artery	Jugular Vein	Rumen Veins Posterior	Rumen Veins Anterior
10	—	—	45	88
11	—	—	55	109
12	—	—	18	65

I may say here that the estimation of the specific acids presented an important problem. Our earliest observations had been carried out by redistillation methods (Friedemann, 1938) but these were checked and indeed for our purpose replaced by a chromatographic method introduced by Elsden.

(3) *Under what circumstances does absorption take place?*

The absorption of substances from the rumen is not a simple process. It might be expected that if acetic and propionic acids were put into the rumen in equi-molecular proportions, they would be absorbed at equal rates, but this is not the case. Among the factors which play a part in the regulation of the rate of absorption, a prominent one is the hydrogen ion concentration. Contrasting the two *p*H's, 7.5 and 5.8 respectively, with regard to the absorptions of acetic, propionic and butyric acids from the rumen, Danielli, Hitchcock, Marshall and Phillipson (1945) believe that at *p*H 7.5 the acids are absorbed entirely as the sodium salts, in which case the acetate is absorbed more rapidly than the propionate and the propionate than the butyrate, but when the *p*H is 5.8, free acid is absorbed from the rumen as well as the salt, in which case butyrate is absorbed more rapidly than propionate and propionate than acetate.

The difference seems to hinge on the condition of the membrane lining of the wall; when *p*H is alkaline, absorption takes place by simple diffusion through water-filled pores of the intercellular spaces; when it is acid absorption takes place, in addition, by the free fatty acid ions penetrating the lipoid coverings of the cells themselves. This is perhaps the place to mention the important work which has been taking place within the last few years in Marston's laboratory, at the University of Adelaide. This work has not yet been published *in extenso* owing to causes con-

nected with the war and the health of the workers, but I am indebted to Dr. Marston for a resumé of it and for permission to quote the same.

"*In vitro* fermentation of cellulose with mixed cultures of micro-organisms withdrawn from the paunch results in the formation of propionic acid, acetic acid, methane and carbon dioxide, providing the milieu approximates closely that of the rumen.

"In 1938 we isolated the fatty acids from each of a series of such fermentations and separated the constituents by fractional distillation. The molecular ratios (propionic) : (acetic) proved to be close to 1.8 : 1, so that very close to 25 per cent. of the combustible energy of the fatty acids arising from rumen digestion would be in the form of acetic acid. Very small quantities of formic acid were also present. Lactic acid invariably occurred in such quantities as to suggest it is an evanescent intermediate in the main degradation."

(4) *What absorbable materials are found in the rumen?*

I might add as a sort of sub-heading; "—and how do they compare with those found in artificial digestions of carbohydrates?"

Artificial digestions have been carried out by Elsden (1945). Cellulose, glucose and lactic acid are rapidly fermented *in vitro* by rumen contents, with the production of acetic, propionic and butyric acids. Table II gives a summary of Elsden's data sufficient to show the general trend of his results.

TABLE II

Fatty acid (mg.) from 500 mg. of cellulose, glucose and lactic acid obtained by inoculation with rumen contents in about 90 hours.

	Cellulose	Glucose	Lactic Acid
Acetic Acid ..	83	52	49
Propionic Acid	147	135	147
Butyric Acid ..	4	50	32
Total Volatile Acid	234	237	228

In his paper Elsden set out the results not in milligrams, but in millimoles, which tends to flatter the acetic acid and depress the butyric.

These same materials, as will have been gathered from what has already been said, are those found in the rumen after a meal, and they are the bodies which comprise the "volatile acids" found in the blood flowing from the rumen. But while qualitatively the acids concerned are acetic, propionic and butyric, quantitatively of the total volatile acid the percentage of acetic acid is greater and that of propionic less—

(a) in the rumen itself,

(b) in the blood coming from the rumen,

than in the digestions of cellulose, glucose and lactic acid given above. Taking them one by one—

(a) So far as the rumen contents are concerned, Table III gives the volatile acids in mg. obtainable from 100 c.c. of rumen content.

TABLE III

Volatile acids from 100 c.c. rumen contents in mg.

Sheep	*Acetic acid*	*Propionic acid*	*Butyric acid*
F	250	71	58
F	152	72	62
F	370	220	158
C	61	56	118
D	352	121	94
E	348	123	180
G	116	42	52
WF ..	69	29	19
WF ..	140	43	51

(b) The blood coming from the rumen according to distillation curves (Barcroft, McAnally and Phillipson, 1944) contains volatile acids in much the same proportions as the rumen itself. Elsden quoted by McAnally and Phillipson (1944) says "using a chromatographic method of separating the acids, found that in the rumen 60% of the volatile acid is acetic, the remainder consisting of propionic and butyric and possibly a fourth as yet unidentified acid. In the blood draining the rumen the acetic amounts to 80% of the total

volatile acid, the remainder consisting of propionic and butyric acids."

Reverting to Table III, the great disparity between the data for individual experiments requires some comment. Indeed it furnishes a major subject for discussion. The happenings in the rumen depend not only on the chemical substances which happen to have been recently swallowed, but upon the flora existing in the rumen at the time. To state the extreme possible views for purposes of argument, one would be that the microflora which a sample of ingested food meets in the rumen, is a function of the sheep, like the composition of the cells in its salivary glands, or the corpuscles in its blood. The other view is that the microflora in the rumen is a function of what the sheep has previously eaten; that certain forms of life thrive on certain dietary commodities, and whether they abound in the rumen depends upon whether those materials have formed the diet of the sheep.

Whilst we would not pin ourselves to the second view so completely as entirely to rule out any influence due to the specificity of the animal, we regard the diet as being the major factor in regulating the nature of the microflora.

Thus, if a sheep be taken in poor condition and fed on a diet of hay sufficient in calorific value but of poor quality, there will be found in the rumen among other things a large diplococcus and streptococcus. These show poor preferential staining with iodine. If now glucose be put into the rumen through a fistula and a sample be taken an hour subsequently, similar cocci will be observed but now they stain blue with iodine. If this sheep be turned over to a rich diet, good clover hay being substituted for the poor hay and the test repeated after 14 days on this diet, there is a change in the micro-organisms of the rumen.

Before the dosage with sugar, the cocci do not show any marked reaction to iodine. This was the case with the poor hay, but after the sugar was given, the cocci were much

reduced in number; instead a fresh organism appeared on the rich hay diet—"a large oval yeast-like organism similar to the schizosaccharomycete which stains brown with iodine." This has already been described by Quin (1943).

And there are other differences; on the poor hay lactic acid was only present in very small quantities at any stage in the digestion, whereas on the rich it rapidly appeared and reached a peak at about seven hours from the addition of the sugar. Moreover on the poor hay the production of volatile acids was a slow gradual affair, whilst on the rich hay the volatile acid, like the lactic acid, reached a maximal concentration at seven hours. Also glucose persists for longer on poor diet than on good diet.

The Metabolic Importance of Volatile Acids

A matter of first importance is, of course, whether the volatile acids produced as the result of bacterial action have any metabolic importance. If they have no metabolic importance, the study which we have described is at best one of academic interest and at worst a waste of cellulose and other carbohydrates which might have been used to some purpose. Here there are two points:

(1) Can volatile acids be metabolised in such a way as to provide the body with energy, and, if so,

(2) Are they in fact produced during ruminant digestion on such a scale as to provide a worthwhile fraction of the energy requirements of the body?

(1) *Can volatile acids be metabolised in such a way as to provide the body with energy?*

Experiments in which acetic acid has been put into the rumen through a fistula show two things:

(a) The acetic acid disappears.

(b) It is not excreted as such.

What then becomes of it? Here much work remains to be done. One proven site of destruction of acetic acid is the beating heart. If Ringer's solution containing acetic acid

be perfused through the isolated heart of the rabbit, the amount destroyed is as shown by Table IV.

TABLE IV

Perfusion of the Rabbit Heart with a Fluid containing Acetate.

Serial No. of Expt.	Weight of heart g.	2-hour period	Acetate introduced at commencement of each 2-hour period mg.	Acetate disappearing in each 2-hour period mg.	Remarks
1	2.80	1st	134	56	—
		2nd	,,	52	—
		3rd	,,	28	—
2	5.36	1st	,,	28	Heart not beating
3	4.60	1st	,,	66	—
		2nd	,,	54	—
4	5.20	1st	,,	36	1.7 mg. acetate recovered from heart
		2nd	,,	34	—
5	4.60	1st	141	54	0.8 mg. acetate recovered from heart
Control experiments in which the heart was replaced by a piece of rubber tubing.					
1	—	—	134	5	—
2	—	—	,,	4	—

This destruction of acetic acid is at least on as great a scale as the destruction of glucose from Ringer's solution in which the glucose is present in strength equimolecular to that of acetic acid in the experiments just described (see Table V).

TABLE V

Perfusion of the Rabbit Heart with a Fluid containing Glucose.

Serial No. of Expt.	Weight of heart g.	*Experimental period* Serial No.	Duration hours	Glucose introduced at commencement of each period mg.	Glucose disappearing in each period mg.
1	4.4	1	2	149	20
		2	2½	,,	11
2	5.8	1	2	,,	21
		2	1½	,,	9
3	4.1	1	2	142	29
		2	2	,,	16
4	2.9	1	2	144	30
		2	2	,,	30
		3	2½	,,	27

On the other hand the destruction of propionic acid does not seem to be nearly so great (see Table VI).

TABLE VI

Perfusion of the Rabbit Heart with a Fluid containing Propionate.

Serial No. of Expt.	Weight of heart g.	No. of 2-hour period	Propionate introduced at commencement of each 2-hour period mg.	Propionate disappearing in each 2-hour period mg.
1	4.4	1	181	12
		2	,,	5
2	3.6	1	,,	17
		2	,,	13
		3	,,	15
3	5.9	1	,,	22
4	4.3	1	,,	7
5	7.2	1	,,	9
		2	,,	5

Comparing the destruction of acetic acid, propionic acid and glucose we arrive at Table VII.

TABLE VII

Amount of Glucose and of Acetic and Propionic Acids destroyed per g. of Perfused Rabbit Heart per Hour.

Substance	Amount destroyed mg.
Glucose	2.4
Acetic Acid ..	5.5
Propionic Acid ..	1.1

As indicating the course of metabolism in the heart, these experiments would all have been much cleaner did not the tissue of the heart contain glycogen. It might be contended that the heart derived its energy not directly from any of the materials investigated, but from the glycogen in its tissues. All that can be said at present on this subject is that the most glycogen which we ever found in a rabbit's heart was 3.9 mg. per gram of heart. Compare this figure with the heart 1 in Table IV—that heart which, over six hours, destroyed 48 mg. of acetic acid per gram of heart, or heart 4 in Table V, which destroyed 26 mg. of glucose per gram of heart in 6½ hours. It seems difficult to suppose that these hearts were beating simply on initially stored glycogen, especially when we remember that the CO_2 produced was shown by Locke and Rosenheim (1907) to be roughly equivalent to the sugar used up.

But even if it be accepted that the beating heart breaks down, or if you like breaks up, acetic acid, it is still not proved that the acetic acid is a source of energy. That proof we sought along three avenues.

(a) Experiments with phloridzin.

(b) Experiments with the respiration calorimeter.

(c) Experiments in which isotopic carbon was put in the carboxyl group of acetic acid perfused through the heart and sought in the CO_2 which emerged.

(a) *Experiments with phloridzin*

These experiments depend upon the argument that the body, being unable to use up glucose once the animal is completely phloridzinised, any sugar which is formed will be excreted. If then volatile acids are turned into sugar in

the body, that sugar should be found in the urine. That the body has been rid of other sources of glucose than that caused by the breakdown of its own tissues, is secured by the withholding of food till a constant ratio of glucose to nitrogen is secured in the urine. The matter is being put to the test by Phillipson at present and so far only a partial answer can be given. As far as propionic acid is concerned, there is no doubt that something like 80–90% of the equivalent of the propionic acid can be recovered as sugar within 30 hours of its administration. With regard to the acetic acid, I should refrain from making any statement about the acetic acid on the basis of phloridzin experiments. Certainly the sugar output after the administration of acetic acid does not rise in the same rapid and clear-cut way as it does after the administration of propionic acid. On the other hand it would not be possible to say that there is no indication of an increased sugar output on the administration of acetic acid to the phloridzinised animals.

Here again let me quote from Marston's manuscript:

"Propionic acid is well known to be converted completely to glucose in the phloridzinised dog. Acetic acid administered under identical circumstances gives rise to no extra glucose but serves to increase materially the rate of energy dissipation. Apparently it is oxidised rapidly in the dog without entering the normal production channels of metabolism and can function merely to spare other fuel when extra heat is required to maintain body temperature in a cold environment."

It is thus by no means simple to reconcile these findings with the fact that ruminants normally deal with large quantities of acetic acid which arise from bacterial dissimilation of cellulose and other carbohydrates, and apparently make some effective use of it, other than as a source of heat. We have recently attempted to illuminate this point.

The rate that energy is dissipated by a resting sheep falls steadily on fasting until it becomes relatively constant on the 4th or 5th day after the last feed and subsequently the

R.Q. and oxygen consumption varies but little over several ensuing days. Thus, in a warm environment and with animals trained to recline with little movement in the calorimeter chamber, it is possible to measure with reasonable precision any changes in energy dissipation which supervene on the introduction of various substances into the animal either parenterally or into the rumen.

"Introduction of 70 gm. acetic acid (= 250 kg. cal.) resulted in a rise of energy dissipation which was equivalent approximately to two-thirds of the extra energy which might be expected if the whole of the acetic acid were oxidised without performing any useful function. The calorific equivalent of propionic acid introduced similarly resulted in a small increase of energy dissipation which was barely significant.

"When a sheep is subjected to prolonged fasting, the nitrogen eliminated in its urine, after some fluctuations during the first ten days, becomes approximately constant for a considerable period—the level of excretion depending on the size and the immediately previous nutritional history of the individual, i.e., on its metabolic rate and on the type of material being drawn upon to provide the energy.

"Introduction of 50 gm. propionic acid (= 250 kg. cal.) into the rumen of a sheep that had attained equilibrium in this way resulted in a very marked reduction of the nitrogen eliminated in the ensuing 24 hrs. The reduction when the energetic equivalent of acetic acid (70 gm.) was introduced was only about one-third of that spared by the propionic acid."

(b) *Experiments with the respiration calorimeter*

The experiments of the Cambridge Unit with the respiration calorimeter are not very far advanced and certainly not in a position in which we could claim that acetic acid put into the rumen of the sheep is used as a source of energy.

(c) *Experiments with isotopic carbon*

In view of the uncertain results of the experiments discussed under headings (a) and (b), the ultimate test seems

to be necessary, namely to perfuse the beating heart with acetic acid containing heavy carbon and seek the isotope in the resultant carbon dioxide. For this we had not the facilities in wartime England, so the Agricultural Research Council asked Dr. Visscher in the University of Minnesota to undertake the test. This he did, assigning Lorber, Lifson and Wood (1946) in his team to the task. The result was quite unequivocal up to a point, namely that in two experiments in which labelled carbon was put into the COOH group of the acid used in the perfusion, 30% of the

TABLE VIII

Expt. No.	Heart weight	Blood volume	Acetate* administered	Respiratory CO_2 Individual collection periods	Respiratory CO_2 mM collected	Respiratory CO_2 Atoms per cent excess C_{13}	Respiratory CO_2 Per cent of administered C_{13} recovered	Administered acetate undergoing net conversion to CO_2	Per cent of total respiratory CO_2 derived from administered acetate†
	grams	cc	mM	mins				mgm/gm of heart hour	
1	13	125	5.6	50.5	1.79	0.32	1.98	1.35	31.5
				72.5	2.11	0.66	4.79		
				80.0	2.52	0.78	6.80		
					1.88‡	0.78	4.38		
							17.95		
2	7.7	90	5.4	58	0.99	0.28	1.00	0.92	19.5
				61	1.46	0.48	2.43		
				40	0.80	0.51	1.43		
					0.65‡	0.51	0.99		
							5.85		
3	No heart	80	3.85	205	0.95	0.00	0.00		

* The acetate contained 5.26 atoms per cent. excess C_{13} in the carboxyl carbon.

† $\dfrac{\text{mM Acetate administered} \times \text{\% administered acetate converted to } CO_2 \times 2}{\text{total mM of respiratory } CO_2 \text{ collected}} \times 100$

‡ Residual CO_2 in blood and ventilating gas at the end of the experiment. The C_{13} in this CO_2 was assumed to be the same as that found in the last collection period.

labelled carbon in one experiment and 19% in the other turned up in the resultant carbon dioxide. There seems then to be no doubt that the body can use acetic acid as a source of energy. How economical a source is another matter and one still to be discovered.

(2) *Are Volatile Acids produced on a significant dietary scale?*

In order to form an estimate of the scale on which volatile acids are absorbed from the rumen of the sheep, it is necessary to know not only the concentration in the blood but the quantity of blood which flows through the rumen. The measurement of the total blood flow through the rumen has not proved possible; it is possible to measure that through an area which represents about one-third of the organ and to regard this as representative. In as much as the part measured is that drained by the posterior vein, and as the blood in that vein is poorer in volatile acids than the blood of the anterior vein (see Table I) the measurement will give an under-estimate of the volatile acid absorbed. Table IX shows the order of figures involved, Barcroft, McAnally and Phillipson (1944).

TABLE IX

Sheep	Blood flow per min. from posterior vein c.c.	Concentration of volatile acid in c.c 0.01 N per 100 c.c. rumen blood	g. volatile acid absorbed per hour
13	87	43	3.9
15	59	55	3.7
16	107	35	3.5
18	126	44	5.3
19	67	35	2.6
29	92	26	2.3

For the reason given and others, we regard the above figures as considerably under-estimating the volatile acids absorbed in the rumen and to this must be added an appreciable quotum for that absorbed from the abomasum and the large intestine. We consider that 5 grams an hour

or 120 a day would not be an over-estimate for the whole alimentary canal. Properly to assess the importance of this contribution, let us compare it with 264 grams of carbon which is given as the quantity of carbon absorbed by a 50-kilo sheep from carbohydrates per day. If we assume that the 120 grams of volatile acid absorbed consists of acetic, propionic and butyric acids in the molecular proportions 3 : 2 : 1 the 120 grams of volatile acid would contain 68 grams of carbon, i.e., about one-quarter of the carbon absorbed by the sheep from carbohydrate sources. This certainly is not a negligible contribution, but that is not quite the whole story, because the importance of the volatile acid channel of digestion consists not merely in the amount of carbohydrate absorbed in this way, but of the amount of cellulose, which may thus be saved from waste.

Let us turn to quite a different attack on the rôle of micro-organisms in the rumen.

III. The microflora providing a basis of food for the individual

There is much work to be done still on this subject. The general thesis is, the growth of unicellular organisms in the alimentary canal is so great as to provide an appreciable amount of food for the animal itself. This conception has no significance except in the case of ruminants because it postulates the incubation chamber in the alimentary canal in which the micro-organisms grow, as being above the parts of the canal in which digestion, notably protein digestion, takes place. The theory is that, the rumen being in practice a thermostatically controlled chamber maintained at a suitable *p*H, the organisms found there grow apace. Very prominent among the micro-organisms are ciliates. These ingest the bacterial forms and passing down the alimentary canal are themselves digested by the sheep. The attractive feature of this conception is the possibility that among the micro-organisms there are some which can synthesise protein, using non-protein nitrogenous substances

for this purpose, and therefore provide the animal with a source of protein over and above what it eats. The subject, therefore, divides itself into two main issues:

(1) What evidence exists of the alleged happenings in the rumen?

(2) Can ruminants subsist satisfactorily when only a part of their nitrogen comes from protein in the food?

(1) *What evidence exists of the growth of micro-organisms which are incorporated in the ciliates in the rumen?*

This subject has been extensively studied by Baker. His method has been that of microscopical examination, a method which he appears to regard as only in its infancy. He (1942) says: "It is clear, therefore, that by the systematic assemblage and examination of appropriate collections of material, a whole field of research is opened up which lies altogether beyond the range of pure cultural methods of investigation. These possibilities will, however, only be realised to the full when the method of direct microscopical examination has been adopted as a routine procedure by a sufficient number of observers."

Baker's studies have been principally on the rumen of the ox. I may give his main points in his own words:

"The rumen population of the ox comprises three main groups of organism.

(a) Protozoa.
(b) Iodophile organisms (see Plate 12).
(c) Aniodophile organisms.

The iodophile species comprise macro and micro forms."

A fixed and free iodophile population are discriminated, the former constantly attached to starch grains and vegetable fragments, the latter in suspension in the rumen fluid. The rumen population was qualitatively independent of the range of diet and of the breed of animal examined. Protozoa play no part in the decomposition of cellulose but digest starch. Decomposition of both starch and cellulose is accomplished by the fixed iodophile microflora which in the process synthesise iodophile polysaccharide. During

the passage through the alimentary canal the rumen population is eliminated, the protozoa by peptic and tryptic digestion, and the iodophile microflora—

(a) by digestion by protozoa,
(b) by autolysis,
(c) by action of digestive enzymes.

By the term "appropriate" collections of material, Baker means samples of rumen content which can be treated with formalin and subsequently examined microscopically and/or chemically. For the collection of these samples Baker employs the following technique—

Dilute rumen content 10 times with 10% commercial formalin. Filter through muslin.

FRACTION A
(Coarse vegetable material contains fixed iodophile forms)

Opaque suspension separates on standing half an hour.

FRACTION B
(Rumen protozoa and some vegetable fragments, fixed iodophile forms)

Suspension on standing another ½ hour.

FRACTION C
(Vegetable debris preponderating, fixed iodophile forms)

Suspension on standing 12 hours.

FRACTION D
(Larger iodophile free forms)

Suspension stable several days then centrifuged.

FRACTION E
(Smaller iodophile free forms)

- - - -

The products of microbial synthesis are held to be assimilated by the alimentary canal. The question naturally arises; granting that iodophile bacteria are attached to the vegetable fragments in Fractions A, B and C, what proof is there that the bacteria assimilate the fragments? This rests upon two contentions.

(a) The micro-organisms may be seen in channels which they have presumably excavated for themselves.

(b) When viewed by polarised light the vegetable structures around these channels are seen to ·alter.

Take the case of starch grains. To quote Baker once more: "In the initial phase the micro-organisms are firmly attached to the surface of the grains upon which clearly outlined enzymatic cavities are excised. The attack proceeds from the periphery to the centre and in the later phases the residue is almost obliterated by adherent micro-organisms. Finally the grains are demolished, an interwoven knot of streptococcal chain marking their original locations. During the course of these changes, double refraction gradually ceases and the original blue-black reaction of the starch to iodine is replaced by a reddish reaction of diminished intensity." So much for the effect of micro-organisms on the vegetable fragments.

Let us pass to that of the protozoa upon the microflora. In Baker's view the protozoa do not directly digest cellulose, but he agrees with previous workers (Trier, 1926, Mangold, 1929) that starch grains are found in the bodies of the protozoa. The protozoa also ingest iodophile micro-organisms.

(2) *Can ruminants exist satisfactorily when only part of the nitrogen in their diet comes from protein ?*

Emphasis has already been laid on one property attributed to the microflora of the alimentary canal, namely *that it can utilise non-protein nitrogen to form protein. It follows logically that in the case of the ruminant in equili-*

brium on a basal diet and one in which all the nitrogen was protein nitrogen, some or all of the protein could be withdrawn and replaced by non-protein nitrogen. To the practical applications of this thesis much thought has been given in Great Britain: doubtless wartime experiments have been carried out in many other countries. I may, however, allude to one carried out by Owen, Smith and Wright (1943) at the Hannah Institute at Ayr, in which it is claimed that—

"Seven Ayrshire cows of average milk yield were used for the experiment. They were first fed on a production ration in which one-third of the nitrogen was supplemented by blood meal, a product which has been shown by Morris and Wright to be of first-class biological significance for milk production. After the animals had received this diet for a few weeks, the blood meal was replaced by its nitrogen equivalent of urea, plus its energy equivalent of pure starch. . . . The milk yield of five of the seven cows was well maintained when the blood meal was replaced by urea. On the other hand if neither the protein nor the urea were given, the milk flow always declined at once. The weight of the cows was well maintained on the urea diet. Of the seven cows, then, five seemed to make use of the urea, one gave an ambiguous result and one seemed to make no use of the urea."

Whether this finding is of any economic value is not for me to say, but it does seem that if the microflora are put "on their mettle" they can contribute something worth while as protein synthesisers.

Microflora in the large intestine

This subject has been much less fully studied than the rôle of microflora in the rumen. Nevertheless, as I have already said, microbial production of volatile acids occurs in all forms of life so far studied, whether carnivorous or herbivorous, and the scale on which it occurs seems to be indicated by the size of the large intestine. That being so, the horse naturally demands most consideration.

TABLE X

Volatile acids in the blood taken from vario[redacted]tions in the alimentary tract c.c. N/100 alkali necessary [redacted] neutralise the volatile acids in 100 c.c. of blood (Average).

Animal	Vessel from which blood was taken				
	Arterial	Alimentary canal			
		Portal	Vein draining		
			Rumen	Small Intestine	Large Intestine
Sheep (14) ..	6	-	38	4	22
Rabbit (4) ..	4	9	*Stomach* --	8	23
Pig (4) ..	5	9	7	11	28
Pony (1) ..	7		--	5	44
Dog	--	--		.	—

The scale on which volatile acids are produced in the large intestine of the horse was to me very surprising and at first people—one of them a very important and influential person—were very incredulous, almost hinting some departure on my part from the strict line of factual veracity. I determined, therefore, to extract as much volatile acid as possible from the total contents of the large intestine of a horse as it would yield. This proved to be about 350 grams of the mixed sodium salts of the acetic, propionic and butyric acids.

Knowledge of the microbial population of the large intestine will probably increase within the next four years. Meanwhile, Baker and Palmer (1946) have published an important work on the flora of the human large intestine. Applying the methods which I have already described, they found "iodophile bacteria in large numbers in the interior and upon the surface of plant structures. A specific relation

to the decobasal d·ion of the vegetable was indicated by the absence of m...-organisms from the surfaces of intact or partially intact muscle fibres also present in the ingesta. . . . Preliminary observations made under the polarising microscope suggest that some of these iodophile organisms are able, as in herbivores, to atta...e...rue cellulose."

The fate of micro-organisms fo...ion in the large intestine requires further investigation; at present it seems that for the most part they do not leave it alive. The possibilities are that they are autolysed or that they are evacuated in the dead condition. In the former case, which Baker stresses, there is the further possibility that the products of the autolysis may be absorbed and be of nutritional value.

The emphasis must be placed in this connexion on one difference between the rumen and the large intestine. The micro-organisms in the rumen pass on to parts of the gut lower down in which they can be digested, those which leave the large intestine are evacuated, though it would appear mostly in a dead condition.

Here again the importance of this population to its host is quite an open matter, as Baker and Palmer point out it may even be deleterious (Lohrisch, 1904). In the horse it surely cannot be so; in the absence of measurements it is only possible to say that if the blood flow through the large intestine of the horse is at all commensurate with the size of that organ, the amount of volatile acid absorbed must be a definite asset to the nutrition of the animal.

If I have given the impression that there are two rival schools of thought in Britain with regard to the microflora of the alimentary canal, let me say at once that any such idea would certainly be premature and probably would be wrong. We at Cambridge have concerned ourselves almost entirely with the final processes and end products of ruminant digestion. Baker's principal concern has been with the initial happenings. We regard ruminant digestion as a process involving at least two stages. We have convinced ourselves that among the final products volatile

acids are conspicuous and that these [illegible] ed by a bacterium of the propionic family. [illegible] acterium probably takes on its task at the stage of lactic acid. The lactic acid we believe to be produced by the action of iodophile organisms. [illegible] ct remains to be seen whether among the happening [illegible] ved by Baker and his colleagues, cellulose is not in pa [illegible] rned into material which in the last resort forms the basis of volatile acid production. It must be remembered, moreover, that in our experiments we have never accounted for all the initial cellulose in terms of volatile acids; usually about half, or a little less, remains unaccounted for. When some deduction has been made for the production of carbon dioxide and methane, there still remains a considerable residue which might be devoted to protein production, and remember the amount of protein which an animal requires is much smaller than the amount of carbohydrate. With the facilities which Baker will have at the Rowett Institute, I look forward to the next ten years.

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GLOSSARY

ABOMASUM (*see also* Rumen). The fourth pouch of the stomach of ruminants, the one where true gastric digestion takes place.

AGGLUTININ: A group of substances in blood which causes a clumping of the red cells, or of bacteria under special circumstances.

ALIMENTARY CANAL: A general name for all the intestines, from mouth to anus, the excretory end.

ANASTOMOSIS: Junction between two tubes; as. two arteries.

GLOSSARY

AUTOCLAVE: A vessel, usually me[illegible] can be securely clamped and seale[illegible] heated, the pressure inside rises [illegible] ti[illegible] atmospheric, and [illegible]m becomes superheated (its temperature high[illegible] 100° C).

CAECUM: The part [illegible]ntestine which is the beginning of the large bowe[illegible]ere the small bowel runs into it The appendix is attached near this point.

CANNULA: A little tube for fixing into a vein to bleed it.

CELLULOSE: The woody part of all plants, the basic skeleton of their cell walls.

CHROMATOGRAPHY: A method of analysing mixtures of substances by taking advantage of the fact that fuller's earth, or aluminium oxide, or certain other adsorbents, attract the constituents of the mixture to differing extents, holding on to some substances much more avidly than others. When a solution of the mixture is allowed to trickle through a glass tube packed with adsorbent, each component attaches itself to the adsorbent at a particular characteristic position along the tube, and so is separated from its associates.

CILIATES: Microscopic single-celled animals (protozoa) which swim by means of the very fine "hairs" or "cilia" with which they are covered.

CRITICAL TEMPERATURE: A gas or vapour can often be liquefied by compressing it: thus liquid air is made. But above a certain temperature, characteristic for each gas, this is no longer possible, and no amount of compressing will liquefy it as long as it is hotter than this critical temperature. Thus steam, for example, will never yield water till it is cooled a little below the critical point.

EPITHELIUM: A membrane formed by a sheet of cells of similar appearance.

FISTULA: An artificial or abnormal opening from some bodily organ to the outside; as, a gastric fistula, a

FRIEDMAN, basal ... the stomach to the skin surface of the
KON, S. K., ...
LOCUC... S., & ... sugar present in blood.

GLYCOGEN: Animal starch, the form of carbohydrate stored in the liver and m... the...

HALIDES: A general chemical name for all salts containing chlorine, iodine, bromine or fluorine.

HYDROCARBON: Any chemical substance composed only of carbon and hydrogen, no matter in what proportions, or of how many atoms.

HYDROGEN ION CONCENTRATION (also written pH): A measure of the acidity of a solution. Values less than 7 are acid (increasingly so, the smaller the number), those greater than 7 alkaline, e.g., acetic acid or vinegar might be 3, bicarbonate about 9.

IODOPHILE: Staining darkly with iodine.

LACTIC ACID: The acid of sour milk, the acid produced by muscles in strenuous exercise; sometimes an intermediate stage in the breakdown of starch and sugars by living things.

LIPOID: Complex fatty material.

MICROFLORA: Plants of microscopic size, often single-celled, and including bacteria and yeasts.

MOLECULAR WEIGHT: Molecules are so small that their actual weights are very slight by everyday standards. It is convenient therefore to compare them on a relative scale, with the lightest atom, that of hydrogen, arbitrarily taken as the unit. The molecular weight of a substance is thus the number of times its molecules are heavier than atoms of hydrogen. This is sometimes a convenient measure of their size.

OBSTETRICS: Study of antenatal care and child-birth.

OPIATE: Pain-relieving drug.

PARAMETER: The name given to a quantity in mathematics when it is a constant in the example considered, but varies in value from case to case.

pH: See hydrogen ion concentration.

PHLORIDZIN: A drug, obtained [illegible] feres with the usage of gluc[illegible] cular, it prevents the kidne[illegible] free of sugar, [illegible] consequ[illegible] appears in [illegible]e bladder in large [illegible]y.

POLYMERISATION: [illegible] certain circumstances, molecules or atoms o[illegible]ame substance can be persuaded to link together t[illegible]rm bigger and bigger molecules, then sometimes called polymers. This process is the basis of the manufacture of plastics, which are made up of molecules of colossal size.

RADICAL: A fragment of a molecule, which behaves as a unity, and may have a brief independent existence in the course of a chemical change.

RIGOR: Sudden uncontrollable shivering attack, when the temperature of the body suddenly rises.

RINGER'S SOLUTION: A solution, devised by Sidney Ringer, of various salts in definite proportions, suitable for keeping tissues alive and behaving fairly normally, e.g., isolated hearts will beat, isolated nerves will carry nerve messages.

RUMEN: The stomach of certain animals (ruminants), which chew the cud, is divided into four deep pouches. The first of these, which the food enters on being swallowed, is the rumen, which is a kind of storage room. Food can be regurgitated from it for further chewing, or allowed to pass into other pouches of the stomach for true digestion to begin.

VOLATILE FATTY ACIDS: A name given to certain acids produced by fermentation, usually of carbohydrates which are (a) capable of being obtained from a solution or mixture by distillation; (b) molecularly, composed of small hydrocarbons with an attached acidic group.

UREA: A simple chemical waste product excreted in large quantity in urine, where it is readily converted to ammonia on standing.

FRIEDMA ... basal ... st...
KON, S. ... -or...
LOCJC H S., & ...sugar ...

...r Contributors

Sir Joseph ..., was ... of the Unit of Animal Physiology of the Agricultural R... Council, and his other appointments filled a column of "...these ...io." Sir Joseph died on March 21st, 1947, while this issue w... press.

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ABOUT OU[illegible]

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Wilson Smith is Professor of Bacteriology at University College Hospital Medical School. He began his research career in 1927 at the National Institute for Medical Research and is best known for his investigations on influenza in collaboration with the late Sir Patrick Laidlaw and Dr. C. H. Andrewes. It was this team which first isolated the virus of human epidemic influenza in 1933. In 1939 he was appointed to the chair of bacteriology in the University of Sheffield but returned to London in 1946 to take up his present position. In addition to his influenza investigations he has worked on several other virus and bacterial diseases, being chiefly interested in the mechanisms concerned in infection and host resistance.

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